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THE OLDEST CIVILIZED MEN.

BY E. D. COPE.

Recent explorations in Babylonia have given us much information as to the characters and customs of the oldest civilized people of whom we have any knowledge. The earlier explorations were conducted by M. de Sarzec, French consul at Bagdad, and the report of his work was issued in a magnificent folio in joint authorship with the distinguished anthropologist of Paris, M. Leon Heuzey, beginning in 1889. A little later the department of Archeology and Paleontology of the University of Pennsylvania sent out Messrs. J. P. Peters and J. H. Haynes to make excavations further south in the Euphrates plain. They selected Nippur or Nufar as the point of research, and work has been continued there from 1888 to the present year. The climate of this place is very trying, and the character of the people dangerous, but Mr. Haynes on whom much of the actual labor fell, obtained an amount of material which in quantity and quality equals that obtained by the museums of London and Paris.

The Philadelphia material has been investigated by Dr. Herman Hilprecht, Professor of Assyrian and comparative Semitic Philology in the University of Pennsylvania, and he

has published two memoirs of great interest in the transactions of the American Philosophical Society, the second of which was issued in the present year. From this memoir and the previous one of De Sarzec and Heuzey I compile a few facts regarding the physical characters and habits of the earliest inhabitants of Chaldea, the Sumerians or Accadians. The information on these points is obtained largely from statues and picture-carvings on tablets of a dark limestone, found by De Sarzec at Tel-lo, and by Haynes at Nippur. The figures of animals of known species are so characteristic as to prove that the artists possessed a true eye for form. We may infer that their delineations of man are equally accurate, and that the conspicuous characters which they exhibit are trustworthy delineations. The general resemblance between the features depicted show that we have to do with an interesting and peculiar race.

In the numbers of the NATURALIST for January and February, 1893, Mrs. A. Bodington gave our readers a sketch of the Sumerian question. She followed the belief which had gained currency at one time, that these people were of Mongolian origin. Others have suggested that they were African. The drawings and statues described by Heuzey and Hilprecht show that these ideas were quite unfounded. I reproduce one of the latter from Hilprecht (Plate XVI, l. c.), which is known as the stele of Ur-Inlil. Ur-Inlil was the high priest (or padesi) of Nippur, and he is represented as making an offering to some god on the upper half of the drawing. On the lower half a goat and a sheep are followed by two men, one of whom carries a vessel on his head, the other carries a stick (Plate XII). Another tablet from Nippur displays the same kind of men, and they are also represented on eleven tablets figured by De Sarzec and Heuzey from Tel-lo.

That these represent a race advanced in civilization is clear. They built temples and palaces on huge plateaus constructed of brick. They carved statues and vessels and made pottery. Especially they left records of their history on numerous cylinders and tablets of clay of which many have been preserved. They formed organized armies armed with spears, bows, and shields. What relation did these people bear to the people

of Nineveh whose monuments were revealed to Europe by the labors of Rawlinson, thirty-five years ago? Heuzey declares them to have been older than the Assyrians, and this position is proven by Hilprecht, who believes their earliest king whose name is preserved in the records of Nippur, Enshagsagana, to have lived 4500 B. C. Many kings intervened between him and Sargon I with whom Assyrian history for a long time commenced. These people were predecessors of the Assyrians of Nineveh, and gave them their cuneiform characters, but they differed from them in customs, and to some extent in language. One marked difference of custom, was the fashion of shaving the hair from all parts of the head excepting the eyebrows. Everyone knows on the contrary that the Ninevites took great pride in their hair, and that both on the calvarium and face it was curled and arranged with great care. The figures also show that the Sumerians did not practice circumcision as most Semitic and some other races have done.

The shaving enables as to get a pretty good idea of the form of the head and face. The skull is oval, rather long and flat, and probably mesaticephalic. The jaws both upper and lower, are remarkably small, giving an extreme orthognathic type The nose is remarkably long, prominent and curved, with a good bridge. The eyes are large, horizontal, and not bridled. The cheek-bones are not large, and in the supposed gods, where the hair remains, and in a few other unshaved portraits, the beard is abundant, and the ends of the hair of the calvarium curled up. The figure of the body is robust, broad and rather short. The extensor muscles, i. e. gluteus, quadriceps, and gastrocnemius are well developed.

From the above it is evident that no thought of Mongolian (=turanian) or Ethiopian relationship can be admitted. After a study of some of the least characteristic heads broken from statues, M. Heuzey remarks, that "the evidence is not sufficient to demonstrate the existence of Turanians in Chaldea." These people are clearly of the great Indoeuropean subspecies of man (*Homo sapiens caucasicus*), so that the question reduces itself to one of the determination of their race position. Are they Aryans, or Semitics? using these two terms as covering all the forms of the greater subspecies to which they belong. In the

determination of these minor divisions of man, physical characters begin to fail us. We can only say that if the term Aryan is used for the western peoples generally, the Sumerians differ from them in the direction of the Semitics by their large oval eyes and hooked noses. On the other hand, the small and delicate jaws are not features of Semitic peoples. But the people of Persia or Iranians, hold very much this intermediate position between the two peoples. We scarcely know the shape of the jaws and chins of the Ninevites for they are never shaved. So far as the visible features go they resemble the Sumerians. It is on all grounds to be supposed that the people of Nippur and Tel-lo are the primitive Aryans of the Iranian or Persian race, and ancestors of the Ninevites.

In any case it is evident that we have in these most ancient of civilized people, a type of man as high as any that has since appeared from the point of view of physical evolution. The extreme orthognathism; the prominence of the nose; the reduction of the cheek bones, the full beard; and the well developed extensor muscles of the leg, prove this. *Homo sapiens caucasicus* had reached his full characters on the plains of the Euphrates 6400 years ago.

The relation of time and race of the oldest civilizations to the prehistoric peoples, is a problem which will doubtless be solved in time. Did the Neolithic people exist in Europe contemporaneously with the Sumerians of Chaldea? The only light that can be thrown on the question is as follows. The Sumerians were not stone people, but bronze people. They had no knowledge of iron. No search has been made for the remains of animals which were their contemporaries, but several species are clearly represented on their sculptures. The most common are the lion and the ox (*Bos taurus*, not the buffalo). There is a good drawing of a gazelle in the collection of the University of Pennsylvania. The goat and sheep represented on the accompanying Plate XII, are species now existing in Persia. The goat is near the *Capra ægagrus* of the mountains of East Persia, the ancestor of the domestic goat; and the sheep is apparently the wild sheep of the same region *Ovis vignei*. So from a paleontological point of view, the Sumerians were quite modern.

ON THE ROLE OF ACID IN THE DIGESTION OF CERTAIN RHIZOPODS.

BY JOHN C. HEMMETER, M. D.,[1]

In the "Annales de l'Institut Pasteur," for 1890 and 1891, there are two papers by M. Felix le Dantec on "Researches on the intracellular digestion among the Protozoa," which are detailed accounts of systematic experimentation concerning the occurrence of acid in the digestive vacuoles of Protozoa.

In 1889, E. Metchnikoff published a discussion of the reaction of plasmodia to ingested litmus, also in the "Annales de l'Inst. Pasteur."

Miss M. Greenwood and E. R. Saunders, in the "Journal of Physiology," Vol. XVI, 5 and 6, 1894, have published an exhaustive account of the function of acid in Protozoan digestion, of which the following brief abstract is considered necessary before proceeding to the original part of this report.

It was found that while these protozoa ingest solid matter constantly and promiscuously, such matter has a determinate fate. If it is innutritious it is ejected after lying in contact with the animal's substance for a length of time which varies with many changing conditions. Nutritious matter, on the other hand, during enclosure in food vacuoles undergoes profound change, and this change is effected by something passed out of the protoplasm into the vacuole, acting in a fluid medium and by its presence making that medium deserving of the name "secretion." In Actinospaerium, also, and in *Amoeba proteus*, digestion in like manner is effected, not by direct contact with the acting protoplasm but by some constituent of a fluid, the formation of which the presence of food alone is potent to bring about. These protozoa depend upon the solution of proteid for nourishment. Starch undergoes no digestive change, and the value of ingested fat globules is doubtful.

The following is a report on the role of acid in these digestive vacuoles. For method of observation, it may be briefly

[1] Phil. D. Etc. Baltimore, Md.

stated, that plasmodia and Vorticellidæ were watched for periods which varied from one to fifteen days; large plasmodia were isolated or preserved in concave slides. Even on plane slides the pressure of the cover slip was slight enough to allow of the emission of short pseudopodia in planes at right angles to the plane of extension of the slide and the animals, by means of pipettes, were transferred to fresh water daily.

In a synopsis of the work of Greenwood and Saunders, in a previous report bearing on this matter, in the Journ. of Physiol., the changes undergone by litmus, Congo red and alizarin sulphate, and the solution of the globoids of aleurone grains, which are composed of a delicate nitrogenous capsule enclosing pure calcium and magnesium phosphate, were described. It was emphasized that the outpouring of acid is unaccompanied by any digestive change on nutritive matter; ingesta may indeed be stored for many hours in vacuoles before they are dissolved, or digestion may follow rapidly on ingestion. But the formation of the digestive vacuole, whether immediate or delayed, is preceded by the development of acid reaction and followed by its diminution. Bearing in mind that litmus is changed from blue to red not only by free acid, but also by unsaturated compounds of acid with the products of digestion, *i. e.*, acid salts. And that Congo red changes to blue in presence of free acid only. It is apparent that the diminution of acid in a digestive vacuole is at first due to a combination with the products of digestion, for at this stage any litmus accompanying ingesta is still red, while Congo red has reverted to that tint from blue. Here free acid is absent but acid salts are present. But later on the vacuoles and ingesta, reddened by litmus, become violet and blue so that finally acid and acid combinations are alike absent. That the acid is at one time free is indicated unmistakably by the striking development of violet colors in solids stained with Congo red. Now as the amount of acid present at any moment must be very small, and this being so, that the change in Congo red should be speedy and striking suggests that it is an inorganic acid but it is probable that to emphasize such an inference would be hasty.

In most of the existing records of Protozoan digestion there are indications that the process shows irregularity in its outset and progress. It is not easy to foretell the immediate fate of ingested matter though of its ultimate fate there may be little doubt. There may be marked inhibition of digestive activity even after free ingestion. In plasmodia ingested nutrient matter may be actually discharged after very imperfect digestion. One of the most puzzling phenomena, however, that has been described by all observers in this field, has been termed by Greenwood and Saunders the stage of storage. This process consists in the preservation of ingested food masses, which on first enclosure have been surrounded by liquid within a vacuole, in a shrunken seemingly very acid state. At times 100 non vacuolate, acid ingesta may lie within the substance of a vorticella, whilst active digestive solution is going on in other food vacuoles at same time.

The storage of nutritious ingesta for hours and days in a condition in which acid indicators give evidence of an acid condition, whilst the same kind of nutritious material will undergo rapid digestive solution in an adjacent vacuole, naturally excites one's curiosity. For a long time I had been looking in vain for some explanation of this phenomenon when an accident gave opportunity of viewing it in a new light.

The plasmodia of a large mycetozoon, most probably *Lamproderma scintillans*, had been under observation for about three weeks. Some of these amoeboid organisms were so large as to more than cover the field of vision when objective D and apochromatic eye-piece, No. 4 of Zeiss were used. They showed a habit of devouring everything in their vicinity in the ditch water in which they were cultivated, as a result of which they were at times so filled with debris that no accurate observations were possible. It was planned to transfer them gradually by pipettes into clearer and clearer water and by starvation compel them to rid themselves of the dirt they contained. This proved successful and after 8–18 days of transferring the plasmodia were in practically clear water, free from algæ, infusoriæ, gregarines, bacteria, etc., and the usual

fauna and flora of ditch water. It was a surprise to find that dried egg albumen stained or ingested with litmus and Congo red, under these new conditions, was as a rule promptly dissolved in the vacuoles, taking from 5–24 hours for completion of the digestive act. The same occurred with stained globoids of aleurone grains of ricinus and with stained torulæ. These experiments were repeated many times on many different individuals, and though food ingesta were occasionally observed in a stage of storage, this was the great exception.

The scarcity of storage vacuoles in such plasmodia that had been kept in clear water for nearly a week and given opportunity to disgorge the debris with which they were loaded was conjectured might be brought about by two factors:

(1) The first was that the process of clearing and transferring them to distilled water (in which they do not thrive as well as in Pasteur's fluid with $\frac{1}{5}$ % Na cl) the organisms had been starved and in a sense were too hungry to store food particles, but went to work at them immediately. There is no method conceivable by which such a supposition could be put to experimental test, for which reason it cannot be contradicted or proved.

The second supposition was that (2) absence of storage vacuoles might be caused by absence of bacteria, for in their normal environment the Protozoa are generally in close company with swarms of *Bacierium termo*, zooglea of micrococci and manifold spirilli and other schizomycetes, and by cultivation they had been brought into an almost aseptic, sterile environment.

The latter hypothesis is capable of experimental testing. For if bacteria will produce the phenomenon of storage then the supplying of septic food will be all that is requisite to add to the sterile solution. As a matter of fact it will be found that this is exactly what will happen. In a plasmodium that had shown 8 storage vacuoles in 24 hours of observation in a solution of $\frac{1}{2}$ % sodium chlorde (in distilled water) in which it had been kept one week, 48 storage vacuoles were observed in the next 10 hours on supplying dried albumen dust, moistened with the zoogloea from a Hay infusion.

Vorticellidæ which take in food particles readily are remarkably free from bacteria in their food vacuoles. Amoeba and plasmodia alike exercise to some extent a selective ingestion. Greenwood and Saunders claim to have watched *Amoeba proteus* for 14 days when surrounded with *Bact. termo,* vibrios and micrococci and the absence of bacteria from the endosarc was remarkable. They are taken in, it would seem, as unavoidable accompaniments of surrounding food only. Bacteria are not recorded to have been observed ingested by protozoa per se.

Another evidence of selective ingestion has been mentioned by Dantec, *l. c.*, as distinguishing between inert and living matter. Active monads or groups of spirilli are placed in marked vacuoles of ingestion, containing much of the acid secretion in comparison to inert matter which is usually invested very closely. We therefore have some evidence for assuming that plasmodia and Vorticellidæ distinguish between inert food and bacteria.

(1) Bacteria are rarely ingested except as unavoidable accompaniment of food. (2) Inert food, free from bacteria, is invested closely. Septic food within wide vacuoles. (3) In sterile environment, food in the stage of storage is the exception; in environment of bacteria, storage in acid vacuoles is frequent. I have brought these facts before you in this incomplete form, because the results are fairly uniform, and with the hope of stimulating further observation of the matter. These studies require no apparatus outside of the microscope and acid indicators. The general suggestion drawn from the result has a wider bearing than one would at first sight assume. For if further study will confirm that the ingestion of bacteria constantly prolongs the stage of maximum acidity from the usual time of 24 hours to several days in rhizopods. The suggestion is that the purpose of the acid is one of (disinfection) killing off bacteria.

There is a general uniformity of opinion that the presence of acid is unaccompanied by any digestive change on nutritive matter, which may be stored for many hours before it is dissolved and Greenwood and Saunders intimate that the endo-

sarc secretes some zymogen which perfects the digestive secretion.

The object to which the acid would seemingly serve in these organisms, which may be said to be on the very threshhold of life is the same which Bunge ascribes to it in man. Bunge's view is that the HCl has no other purpose than the sterilization of food. " Why should a chemical substance be placed in the entrance to the digestive tract," he asks, " in exactly the strength necessary for the destruction of bacteria which is directly antagonistic to the chemical reaction in which the main work of digestion must be carried on ? The proteids are more readily converted into a solution lower down in the intestine and in an alkaline medium than by pepsin and acid. The object of the acid is, according to him, then, one of sterilization. This view cannot be denied, at the same time it must be admitted that HCl serves also a digestive purpose.

In the Rhizopods experimented upon, the observations of Greenwood and Saunders could be confirmed concerning the fact that while the acid is secreted in the food vacuoles under the stimulus of all ingesta ; the true digestive vacuole which occurs only under the stimulus of nutritive matter apparently contains something besides an acid, perhaps an enzyme. The change in the acid indicators is as regards time and intensity of color transformation to all observation alike. There seems to be the same amount of acid in a storage vacuole as in a vacuole causing active solution of proteid matter, in close proximity to it, hence the assumption of an additional zymogenic substance in the latter is justifiable. As the amount of acid in one of these vacuoles is very small, and the change in Congo red to blue is speedy and striking, lends belief to the suggestion of Greenwood that the acid is an inorganic one. Why the protoplasm around a storage vacuole will not secrete zymogenic matter, though acid is clearly present in it, and at the same time this enzyme must be accepted to be present in a vacuole in which, close to the former, active digestion is going on is a question difficult to approach. If it can be demonstrated that all or most storage vacuoles contain some substance, living or inert, which is hostile to the economy of the Rhizopod and against which it protects itself by intensely acid

investment of the enemy for a prolonged period, a new and interesting light will be thrown on this phenomenon.

In the "Centralblatt für Bacteriologie, Parasitenkunde u. Infektions krankheiten, Vol. XIX, p. 785, Dr. C. Gorini describes a method for cultivating *Amoeba zymophila* on a solid medium which in this case is the potato. It is certain that Amoebae will grow on old and new potatoes with alkalinization. This would offer an easy and convenient method of cultivating them. It should be emphasized that it is almost impossible to produce cultures of amoeba that are absolutely free from bacteria. A. Celli in the Centralbl. f. Bacteriologie, Bd. XIX, p. 537, describes a number of futile attempts to obtain such cultures. For our purpose it is not essential that the amoebic cultures should be absolutely free from bacteria, a relative, approximate sterility is sufficient to demonstrate the scarcity of storage vacuoles in the amoebae and plasmodia in such environment. Celli's favorite solid medium is a preparation made from Fucus Crispus with 5 per cent Sterilized Water, with or without Bouillon, but always made alkaline. To 10 c.c. culture medium, 1 c. c. of an $\frac{N}{10}$ Solution of Potassium hydroxide or 4–5 c. c. of a saturated solution of Sodium Bicarbonate. This culture medium of Fucus after it is made in the manner that Agar is generally prepared solidifies readily.

In the same Journal, Centrbl. für Bacteriologie, Band XIX, p. 258, Dr. M. W. Beyerinck describes a solid medium for amoebic cultures made from solidified agar by diffusion of the soluble organic substances in it into superimposed distilled water, which process requires about two weeks and repeated sterilization and subsequent addition of salts suitable to formation of nitrites.

I have no experience with these methods and have always found that for my purpose a solution of a little wheat bread in distilled water kept in a small flat dish under a glass cover was all that was required to have Amoeba and plasmodia of mycetozoa constantly on hand. The dish must be kept on a little earth and not in too bright a light and at a constant temperature. This simple culture medium, which of course is unsuitable for pure cultures was suggested by Prof. Reichert of the University of Pennsylvania.

THE BACTERIAL DISEASES OF PLANTS: A CRITICAL REVIEW OF THE PRESENT STATE OF OUR KNOWLEDGE.

By Erwin F. Smith.

I.

It is scarcely fourteen years since Dr. Robert Hartig declared that there were no diseases of plants due to bacteria.[1] Two years later Dr. Anton de Bary, unquestionably one of the most learned and critical botanists the world has ever known and the foremost student of cryptogamic plants, expressed the belief that bacterial diseases of plants were of rare occurrence, and suggested as a partial explanation the fact that the tissues of plants generally have an acid reaction.[2] In his *Vorlesungen über Bacterien*, published in 1885, he expresses much the same opinion,[3] and cites only four diseases, viz., Wakker's hyacinth disease, Burrill's pear blight, Prillieux's rose red disease of wheat grains, and the wet rot of potatoes, described by Reinke and Berthold. Concerning the first of these four diseases he says: "Successful infection experiments and exact study of the life history of the bacterium are still wanting." Respecting the second he contents himself with briefly summarizing the statements made by Prof. Burrill. Of Prillieux's micrococcus he says: "Its importance as a cause of disease cannot be determined with any certainty from the brief account. It

[1] "Für die Krankheitsprocesse der Pflanzen kommen sie durchaus nicht in Frage, etc." Hartig: (*1*) *Lehrbuch der Baumkrankheiten*, 1882, p. 27.

[2] "Bacteria parasitic on plants have scarcely ever been observed, a fact to which R. Hartig has already drawn attention. One reason for this may be that the parts of plants have usually an acid reaction." De Bary: (*2*) *Vergleichende Morphologie und Biologie der Pilze Mycetozœn und Bacterien*, 1884, p. 520; English ed., p. 481.

[3] "According to the present state of our knowledge parasitic bacteria are of but little importance as the contagia of plant diseases. Most of the contagia of the numerous infectious diseases of plants belong to other animal and plant groups, principally, as already noted, to the true fungi." De Bary: (*3*) *Vorlesungen ueber Bacterien*, 1885, p. 136.

may turn out to be only secondary, appearing as a saprophyte in consequence of injuries previously received." Concerning the wet rot of potatoes he states that ordinarily it is a secondary phenomenon following the attacks of the parasitic fungus *Phytophthora infestans*, but admits that exceptionally potato tubers may become wet rotten without the presence of *Phytophthora*, and that "the above named observers succeeded in producing the appearance of wet rot in sound potato tubers by inoculations with their bacteria; in agreement with which stands a recent experiment of van Tieghem, who succeeded in totally destroying living potato tubers by means of *Bacillus amylobacter* when he introduced this into the interior of the tuber and maintained the same at a high temperature (35°)."

In the second edition of his *Lehrbuch*, published in 1889, Dr. Hartig modified his statements somewhat, expressing essentially the same opinions as de Bary. The yellow rot of hyacinths is recognized as a bacterial disease, although rather doubtfully in as much as it is said not to attack sound, well-ripened bulbs, under normal conditions, but only when they have received wounds or been attacked by fungi, especially by a hyphomycete which is said to be an almost constant accompaniment of the rot. The wet rot of potato tubers is admitted to the list, but with the statement that it is mostly a secondary matter, following the rot of stem and cells due to *Phytophthora infestans*. One other bacterial disease is mentioned, viz., pear and apple blight, with the suggestion, however, that it may have been erroneously attributed to bacteria, since the fungus *Nectria ditissima* produces in the bark numerous little bacteria-like gonidia.

Such was the general opinion on this subject down to within less than a decade. Even to-day the majority of well educated botanists would find nothing to contradict in the statement that there are very few diseases of plants distinctly attributable to bacteria. As a matter of fact, however, there are in all probability as many bacterial diseases of plants as of animals.

Various explanations have been advanced to account for this freedom or supposed freedom of plants from bacterial parasitism. As we have already seen, de Bary was inclined to ascribe

it in good part to the acid reaction of vegetable tissue. Dr. Hartig's view is best expressed in his own words:[4] "Whereas the processes of decay, and most of the infectious diseases of man and animals, may be traced to bacteria, the plant organism is protected against them by the peculiarity of its structure, and especially by the absence of circulatory channels for conducting the nutrient fluids which could serve to distribute any lowly organisms which might happen to be present in the food. It is only by means of the vessels and intercellular spaces that they can distribute themselves in any great numbers in the body of the plant, for in other cases they have to pass through the cellulose or woody cell walls, which offer great resistance to their attack. In addition to this, the vegetable juices, most of which show an acid reaction, are unfavorable to their growth. As a matter of fact, bacteria have hitherto been found only in the tissues of plants whose cells are parenchymatous in character and possessed of very delicate walls, as for instance, bulbs and tubers."

For several years Ph. van Tieghem experimented with one or more, probably several, bacteria, called by him *Bacillus Amylobacter* and believed to be the specific agent in the decomposition of cellulose. In 1879,[5] he stated that all the cells of all plants are equally dissolved by it in the meristematic stage but that as soon as the tissues have become differentiated profound differences are noticeable. The cellulose of many plants is dissolved by it but that of mosses, sphagnums, hepatics, lycopods, fern leaves, and stems and leaves of phanerogamous aquatics proved resistant. This behaviour of water plants is "une nécessité d'existence." In 1884,[6] he made a number of additional similar statements. The tubers of the potato, the seeds of beans (first swelled in water and then inoculated directly into the substance of the cotyledons), and the fruits of cucumbers and melons rotted quickly when infected with this organism. Inoculated leaves of Crassulaceæ and stems of Cac-

[4] Hartig: *Lehrbuch*. 2nd. Edition. English translation, p. 37.

[5] Van Tieghem: (*4*) Sur la Fermentation de la Cellulose. *Bull. de la Soc. Bot. de France*, 1879, pp. 25 to 30.

[6] Van Tieghem: (*5*) Développement de l'Amylobacter dans les plantes à l'état de vie normale. *Ibid.*, 1884, pp. 283–287.

taceæ resisted until plunged under oil when they decayed quickly. Aquatics resisted: "By means of a Pravaz syringe I have injected a drop full of the spores of Amylobacter into the lacunary system of several submerged aquatics (Vallisneria, Helodea, Ceratophyllum) but always without result. The plant remained healthy in all its parts."

These papers of van Tieghem are often cited, but they have little substantial value. Undoubtedly he believed that he was experimenting with pure cultures, or, at least, that the results obtained were due to *Bacillus amylobacter*, but such is, to say the least, very improbable. *B. amylobacter* is now believed to be strictly anaerobic, and incapable of any action on cellulose.[7]

More recently Julius Wiesner has divided all plants into two classes, ombrophobic and ombrophylic plants, according as they are or are not readily injured by prolonged rains or exposure to stagnant fluids.[8] His experiments show that the aerial parts of some plants rotted very quickly when exposed to continuous artificial spray while similar parts of other plants proved very resistant, remaining sound for weeks (62 days in case of *Tradescantia guianensis*). The same contrast was observed when leaves of the two sorts of plants were placed in stagnant water, the former lost their turgor and rotted in a few days, the latter proved much more resistant. Many land plants have this power of resistance and all water plants, also all underground parts, even the roots of plants having very susceptible foliage. As additional confirmation Wiesner states that when meat infusions are left to themselves they always decay much sooner than when fragments of ombrophylic plants are placed therein. Ombrophobic plants in water or meat infusion also decay less rapidly when mixed with fragments of ombrophylic plants than when left to themselves. This decay is more rapid in the dark than in light, especially

[7] Prazmowski: (**6**) *Untersuchungen ueber die Entwickelungsgeschichte und Fermentwirkung einiger Bacterien-Arten.* Leipzig, 1880, pp. 23–37.

[8] Wiesner: (**7**) Ueber ombrophile und ombrophobe Pflanzenorgane, *Sitzungsb. K. Ak. d. Wissenschaften*, Math.—Naturw. Classe. Wien., 1893, Bd. 102. Abt. I, pp. 503–521. See also Wiesner: (**8**) Pflanzenphysiologische Mittheilung aus Buitenzorg (III). Ueber den vorherrschend ombrophilen charakter des Laubes der Tropengewächse. *Ibid.*, 1894, Bd., 103, pp. 169–191.

bright light. The foliage of ombrophylic plants is easily wetted; that of ombrophobic plants is as a rule not readily wetted, being usually protected by bloom or some other device for warding off water. When ombrophobic plants are not protected in some such manner, decay is remarkably rapid. In general if the leaves of a plant are readily wetted, it may be assumed that they are ombrophylic, but there are exceptions, e. g. the potato and tomato. Roots of all plants are extraordinarily resistant. In most plants middle aged leaves are least susceptible to decay but in the potato the youngest leaves resist best. Old leaves lose this power of resistance. Sometimes this resisting power is variable in different individuals of the same species, depending on the conditions under which they have been grown. Curiously, all plants of shady, damp places are ombrophobic, if they possess leaves which are not readily wetted, e. g. Impatiens. The more the parts of a leaf are divided the quicker the decay. The green parts of the following plants are mentioned as particularly susceptible to bacterial decay: *Solanum tuberosum, Lycopersicum esculentum, Xeranthemum annuum, Impatiens nolitangere, Chenopodium album, Veronica buxbaumii, Viola arvensis,* and *Taraxacum officinale* (from sunny, dryplaces) *Mimosa pudica, Pisonia alba.* The following plants were found to be very resistant: *Ranunculus aquatilis, Lemna minor, Lysimachia nummularia, Begonia magnifica, Tradescantia zebrina, T. guianensis, Selaginella sp.* (from green house), and *Scolopendrium officinarum.* Among underground organs the roots of the yellow beet proved most resistant. The author's general conclusion from these experiments is best expressed in his own words: "It can now be stated as highly probable that the power of ombrophilous organs to resist rain for months is referable chiefly to the fact that antiseptic substances are produced in the tissues of the organs." These experiments are interesting but seem to have been performed in a rather crude way. The relative rapidity of decay was determined by appearance and the sense of smell and the organisms inducing this decay were undetermined. These experiments should be repeated and extended by Dr. Wiesner

or by some bacteriologist, using pure cultures and plant juices which have been sterilized by filtration.

Dr. Russell's experiments[9] were made a year earlier than than those of Wiesner and have the merit of being properly performed, i. e. with sterile juices and pure cultures so that the conditions under which the experiments were made can be reproduced by other investigators. They are, however, too limited in number to afford any basis for a general conclusion. He found that Canna juice, sterilized by filtration, exerted no appreciable germicidal effect on any of the following species: Kiel-water bacillus, *B. lactis-ærogenes, B. coli-communis, B. megaterium, B. prodigiosus.* Experiments with *B. megaterium, B. butyricus, B. coli-communis, B. pyocyaneus,* and *Streptococcus pyogenes,* using as a culture medium root-pressure juice obtained under sterile conditions from the severed stem of lima beans and Pelargoniums led to a similar conclusion and to the enunciation of the following general statement: "vegetable cell juices, aside from their acid reaction, are entirely powerless against bacteria, and do not possess any germicidal properties like the blood serum of animals."

The old view that plants are not subject to the attacks of bacteria simply because their tissues are acid was shaken by the discovery that some bacteria grow very well in acid media, and was thoroughly upset by the discovery that the juices of some parts of many plants are alkaline. In all probability plants like animals require a delicate balance between acid and alkaline and a continual change from one side to the other for the carrying on of the life processes. Three things at least are certain (1) It will not do to assume that all parts of a plant are acid because some part of the parenchyma shows a strongly acid reaction; (2) It cannot be stated that any given microorganism will thrive only in alkaline media until this fact has been determined by direct experiment; and (3) Many bacteria, perhaps all, are alkali producers and capable, if they can gain any foothold whatever, of slowly changing an unsuitable acid medium into one more alkaline and better adapted to their use.

[9] Russell: (*9*) *Bacteria in their Relation to Vegetable Tissue.* Thesis. Johns Hopkins University. 1892, 8vo. p. 41.

Wiesner's hypothesis is somewhat different. It has been known for some time that various essential oils and other vegetable products, e. g. thymol, salicylic acid, benzoic acid, tannin, quinine, oil of cinnamon, oil of peppermint, etc., exert a powerful restraining influence on the growth of many bacteria, and it is not improbable that a great variety of bactericidal and protective substances occur in plants. On the other hand there may be and probably are bacterial parasites capable of thriving in the very plants which Wiesner found most resistent to continuous spray, to the saprophytic bacteria of stagnant water, and to those of decaying meat infusions, the exact conditions under which any given microorganism will thrive being determinable only by experiment. It must also be remembered that the physiological requirements of bacteria often become profoundly modified to suit changed environments, and that all parasites have undoubtedly descended from saprophytic forms. Prof. Wiesner has, however, opened up a very inviting field and its further investigation by some careful experimenter, trained in bacteriological methods, might lead to very interesting discoveries.

Most of the recent books on vegetable pathology devote a chapter to the bacterial diseases of plants, but these books have not been written by bacteriologists and consequently the statements given are usually very meager and unsatisfactory, and forcibly illustrate the fact that no one can write acceptably on a subject with which he is not familiar, not even if he possesses a logical mind and has read all the "authorities." Excepting Prof. W. Migula, who reviewed the subject briefly but somewhat carefully in 1892,[10] and Dr. H. L. Russell, who gave a brief summary in tabular form the same year at the end of his Thesis,[11] no one seems to have gone over the field critically since de Bary's time, although there is now a considerable body of literature. It is proposed, therefore, in the following pages to examine the literature of this subject from the standpoint of the

[10] Migula: (***10***) *Kritische Uebersicht derjenigen Planzenkrankheiten, welche angeblich durch Baktrien verursacht werden.* Semarang. Midden-Java. 1892. Exp. Sta.

[11] Russel: l. c., pp. 36–41.

modern bacteriologist, sifting as far as possible the wheat from the chaff, and arranging all in an orderly way for convenient reference. The utility of such a piece of work, if well done, can scarcely be questioned, since it must set into sharp relief the gaps in our knowledge and tend to stimulate further research.

The work of the early investigators already mentioned was done before the perfection of modern methods of bacteriological research, and in a time of general scepticism which some of us well remember. It is therefore in no way discreditable that many of their conclusions should be found untenable when tested by the more rigid requirements of the science of to-day. They worked under great difficulties and did as well as could be expected even of men of genius, better, indeed, than many of us would have done. Certainly, as pioneers in a difficult field they deserve great credit.

As much cannot be said for some of the more recent workers who with every opportunity in the way of literature, including numerous manuals of bacteriology, and with laboratory facilities for learning the fundamentals of bacteriological research on every hand in every land, have been content to publish second and third class work, exactly like that preceeding the discoveries of Pasteur and Koch and the development of modern methods. One might suppose these people to have been in a deep sleep for the last twenty years, they take so little note of what has been going on. I shall have frequent occasion to consider papers of this class in the course of these pages and shall not fail to point out their worthlessness, to discourage imitators, if for no other reason. It goes without saying that such publications do not advance science, nor in the end in any way contribute to the reputation of the individual. They are thoroughly discreditable, and in case new species are erected, are little less than criminal, considering the present overburdened and chaotic state of systematic bacteriology.

Thanks to the itch for species making, systematic mycology is generally cited as the most desperately confused and perplexing branch of natural science, but mycology is a highway turnpiked and provided with arc lights in comparison with the wilderness of systematic bacteriology. Of the thousand or

more forms which have been studied and named, or designated by letters or figures or vernacular names,[12] probably not one-tenth can be identified with any certainty owing to the meagerness of the descriptions. The older descriptions are particularly bad, and the effort to decide what was meant by these old names, for which somebody will by and by be strenuously claiming inalienable rights of priority, is usually time thrown away. There is quite enough to do in bacteriology, as every one knows who is dealing at first hand with its hard problems, without wasting precious energy in striving to guess what was meant by two and three line descriptions. All descriptions which do not *describe,* and there are many such, ought to be wholly ignored, and no species recognized as worthy of a place in literature unless so characterized as to be identifiable by others. A plea of this sort in the higher branches of botany or zoology would be a subject for laughter. Bad descriptions are however, so much the rule in bacteriology that it is no laughing matter but rather a great evil urgently demanding reform. It is a state of affairs which has come about naturally enough considering the way in which bacteriology has developed[13] but which would not now be tolerated for a moment in phanerogamic botany or in most branches of zoology and the continuance of which in bacteriology it is incumbent on every honest worker to limit and discourage in all possible ways. The best way in science, always, is to speak out plainly, and to join hands for the advancement of a good cause. Bad work should be ignored and "new species" relegated to limbo unless the descriptions conform to the requirements of modern bacteriological science, meaning by this expression the consensus of opinion among experienced and careful investigators everywhere. If there were some such agreement among the better class of workers, the improvement in systematic bacteriology would become very marked. The volume of publication would, indeed, decrease noticeably but this of itself

[12] About 650 species are mentioned in (***11***) Schizomycetaceæ, by de Toni and Trevisan in Saccardo's, *Sylloge Fungorum,* VIII, published in 1889, but this is not complete.

[13] All the early systematists built upon a foundation of sand, i. e. upon pure morphology.

would be a great advantage, and the quality of the work would more than correspondingly improve. Only in some such way can the strong tendency toward trashy publication be eliminated and light and order evolved from the present chaos.

With few exceptions, vegetable pathology seems to have been specially unfortunate in the class of persons who have devoted themselves to the study of bacterial diseases. While the bacterial side of animal pathology has had its Pasteur and Koch, its Esmarch, Hueppe, Flügge, Gaffky, Frænkel, Pfeiffer, Lœffler, Duclaux, Metchnikoff, Chamberland, Roux, Welch, Sternberg, Smith, Prudden, and a host of other skilled experimenters, scarcely less eminent, and has made correspondingly great progress, the study of the bacterial diseases of plants has been principally in the hands of botanists without special laboratory training in bacteriology and even destitute in some cases of an elementary knowledge of right methods of work. The great development of modern bacteriology is attributable largely to the discovery that human diseases are due to these organisms, and to its consequent alliance with medicine, but there is no reason why the same rigid scrutiny of methods and sharp calling in question of statements which have led to such brilliant results in animal pathology in recent years should not be applied in the same way to vegetable pathology. Accurate experimentation and trustworthy results are from a purely scientific standpoint quite as desirable in one field as in the other.

Two things are especially to be kept in mind in describing any bacterial disease of plants: (1) The pathogenesis must be worked out conclusively; (2) If the organism is named, it must be so described that it can be identified by any competent bacteriologist no matter where it is found.

The four requirements under the first head, i. e. *Pathogenesis*, are now generally recognized to be as follows:

A. Constant association of the germ with the disease.

B. Isolation of the germ from the diseased tissues and study of the same in pure cultures on various media.

C. Production of the characteristic symptoms of the disease by inoculations from pure cultures.

D. Discovery of germs in the inoculated, diseased tissues, re-isolation of the same, and growth on various media until it is determined beyond doubt that they are identical with the organism which was inoculated.[14]

Not one of these steps can be omitted. Possible sources of error beset the investigator at every step, and anything short of a rigid demonstration cannot be accepted as proof. A. is usually quite easy, involving only the careful microscopic examination of abundant material, stained and unstained. B. was made possible by the improvement of methods, *i. e.* by the use of solid media, and especially by the discovery of the method of isolation by means of plate cultures. C. is quite easy, provided the right organism has been obtained and this be inserted into the proper tissues under the right conditions to insure growth. The fulfillment, however, of these conditions often involves long and vexatious delays, and taxes the acumen of the investigator to the utmost. D. serves as a check on all the preceding work, showing that there has been no unintentional mixing of organisms, and that the results obtained were actually due to the supposed cause. For the sake of brevity these four rules of practice will be referred to in the following pages simply as A. B. C. and D. What weight shall be given any specific statement depends of course on the reputation of the individual. Some men are so careful of their reputation and so little given to making unwarranted statements that their simple word goes a long way even when the statements themselves seem improbable, whereas the elaborate explanations of other men, if the asserted facts are at all out of the ordinary, have to be taken with a grain of salt.

The requirements under the second head, i. e. *Description of the organism,* are more numerous, and are embraced under two general divisions of very unequal value, namely *Morphology* and *Biology.* In the classification of the higher plants and animals morphology has been accepted from time immemorial

[14] A series of successful reinoculations is always very desirable and becomes indispensable in case the infections are obtained on plants grown in a locality where the disease prevails naturally. Of course, numerous un-inoculated plants, known as "checks" or "controls," must always be reserved for comparison.

as answering all the requirements of systematists, but such is far from being the case when it comes to the description of bacteria. These minute organisms, which are among the lowest and simplest forms of living things yet discovered by man, are, within the commonly accepted generic limits, so morphologically similar as very often to be indistinguishable with any certainty even under the highest powers of the microscope. As supplemental, therefore, to morphology, and even in many cases as a complete substitute for it, we must have recourse to *Biology*, viz. to the behaviour of the living organism under a variety of known, artificially prepared conditions, such for example as the peculiarity of its growth on various culture media, its thermal death point, its ability to ferment various sugars, the chemical products of its growth, its pathogenic power, etc. Morphologically identical organisms often differ so widely and constantly in their biological peculiarities that there can be no question about their being distinct species, or as to the real value of this means of classification. Probably it also has value, hitherto overlooked, for the differentiation of higher plants and animals, especially for determining the limits of polymorphic or closely related species.

It is not my intention in this place to mention all the biological tests which should be applied to any species for its proper characterization. These are being added to constantly by an army of trained workers in all parts of the world, and my own views of what is at present necessary, or at least highly desirable, will be sufficiently evident in what is to follow. Very likely, also, as knowledge increases, some of the tests which are now generally held to be important will be shown to have little specific worth.

This, however, appears to be a good place to insist on accuracy in all the details of bacteriological work, especially in the preparation of culture media, and on explicitness of statement so that other investigators may know just what was done and how it was done, and thus be able to repeat the experiment. When all details of work are suppressed the inference, naturally enough, is that the writer was ignorant or else that he desired to conceal something not specially to his credit, and which if

plainly expressed might millitate against the value of his work. Either horn of the dilemma is equally bad. Some, however, who are desirous of doing good work in this field, or at least appear to be conscientious workers in other lines, do not seem to be aware of the necessity for extreme care in the preparation of culture media and the management of cultures. As a matter of fact, many bacteria are extremely sensitive to slight changes in the composition of the media in which they are grown or to other conditions within the control of the experimenter, and this appears to be true especially of the pathogenic species. Hence the many conflicting statements about the same organism. A few examples will render my meaning plainer. The careless exposure of cultures to bright sunshine may destroy the organism. An organism supposed to come from diseased tissues or from a culture, and which is being examined in a cover glass preparation, may have been derived actually from a contaminated staining fluid. The apparently simple matter of slightly unclean test tubes or flasks may lead to error, e. g. antiseptic influences may be at work, or preciptates may be thrown down and subsequently mistaken for bacterial growth. Some kinds of glass are unsuited to delicate bacteriological work, the culture fluids being contaminated by substances dissolved out of the walls of the beakers, tubes, and flasks. Tyros, of course, are liable to mistake almost anything for bacteria or to find them anywhere (See a long paper by Bernheim on (*12*) Die parasitären Bacterien der Cerealen, in *Münch. med. Wochenschrift*, 1888, pp. 743–745 and 767–770, and comments on the same by Buchner and Lehmann, *Ibid.*, 1888, p. 906, and 1889, p. 110). Boiling culture media, after it has been compounded, in open beakers or in loosely plugged test tubes or flasks may unwittingly lead to its concentration. The use at different times of different peptones, or grades of gelatine, of unlike per cents of gelatine or agar, of varying grades of acidity or alkalinity, of impure chemicals, of different concentrations of the nutrient media, and of different methods in its preparation all tend to render comparative studies impossible. A large source of error in the differentiation of species by means of sugar fermentation experiments has been the employment of bouillon

containing undetected muscle sugar. Even when preliminary tests are made with some gas-producing bacillus there is still an opportunity for error, provided the tests are carried on only for a day or two. No bouillon should be judged free from sugar and safe for use until in fermentation tubes it has been subjected for at least a week to the influence of *Bacillus cloacæ* or some other organism producing an abundance of gas from grape sugar. If at the end of this period no gas has developed, and the transfer of a loop of fluid from such a tube into another fermentation tube containing a dextrose-bouillon sets up an evolution of gas, then the first bouillon may be used with confidence. Again, if cane sugar is sterilized in an acid bouillon at least a part of it is *inverted*, i. e. changed into dextrose and fructose, and fermentation results obtained therefrom may be due to the presence of any one of three sugars. Bouillon should always be made distinctly alkaline before cane sugar is added. Many of the older fermentation experiments are worthless on account of neglect of such precautions, to say nothing of some recent ones. Again *Bacillus tracheiphilus* grows not at all or feebly on nutrient gelatine as ordinarily made, or in media which is acid beyond a determinable slight degree, and if only such media were used the erroneous conclusion might be reached that it could not be grown outside of the host plant, whereas it grows freely in artificial media, even on gelatine, when the right conditions are established. *Bacillus amylovorus* grows well in some gelatines and refuses to grow in others. Even comparatively slight changes in the acidity or alkalinity of the culture media often have a marked effect on the growth of certain organisms, while others, *e. g.*, *Bacillus cloacae*, are able to grow in almost any medium. Many bacteria prefer alkaline media, and some are very sensitive to the presence of acids, while a variety of bacteria commonly met with in water will not develop at all if the medium is rendered strongly alkaline. Other organisms grow well in acid media.[14a]

[14a] For a striking illustration of the effect on the growth of water bacteria of comparatively slight charges in the reaction of gelatine, see a recent table by George W. Fuller, in a paper entitled: (***13***) On the proper reaction of nutrient media for bacterial cultivation.—*Journal of the American Public Health Association*, Concord, N. H., Oct., 1895, p. 393.

Even the slightly varying acidity of steamed slices from different potato tubers may exert a marked effect on the growth of certain sensitive organisms. On this account some bacteriologists have advised discarding the potato altogether. I have myself found the potato a very useful substratum for the growth of both fungi and bacteria. All comparative tests on potato ought, however, to be made on cylinders or slices cut from the same tuber, and in every case the reaction, acid, neutral, or alkaline, should be carefully recorded. The behavior of the organism on a variety of tubers should also be determined, before deciding that it is something new. It has been thought by some that the best nutrient substance for a parasite must be, unquestionably, the juices of the host plant but this does not follow since there are all grades of parasitism, and even if it did, there are several chances for error in its employment, e. g. the nutrient juices are usually sterilized by steam heat and this may cause a number of chemical changes resulting in a compound very different from the living plant and entirely unsatisfactory as a culture medium, as many have learned by experience. Again, for some particular reason, even the juices of the plant when sterilized at ordinary temperatures by filtration, may be less well adapted to the needs of the parasite than well made beef bouillon or ordinary nutrient agar. In general, the standard culture media of bacteriology should be tried first. Some bacteria can be cultivated only on special media or at special temperatures, or under unusual conditions. *Bacillus subtilis* will only grow in the presence of free oxygen; *Bacillus amylobacter, B. tetani,* and *B. carbonis* will only grow in the absence of oxygen. Winogradsky states that his nitrifying organism obtained from European soils will not grow in the ordinary culture media and thrives best in solutions of inorganic substances, and on silicate jelly. *Bacterium tuberculosis* can be cultivated only in bouillon and on blood serum and nutrient glycerine agar, and at temperatures above 30°C. *Bacterium influenzæ* also flourishes at blood heat and can only be grown, it is said, in the presence of red blood corpuscles or in media containing yolk of eggs; other organisms have thus far refused to be cultivated at any temperature or on any artificial medium, e. g. *Bacterium lepræ* and *B. syphilitis.* Some bacteria

are destroyed at temperatures at which careless workers frequently pour their agar plates, while others refuse to grow at ordinary temperatures or even at blood heat, grow best at 50°–60°C., and are not killed until the temperature exceeds 70° or even 75°C. Finally, a race of *Bacterium anthracis* incapable of producing spores has been developed by growing the organism in media containing phenol; another non-virulent race bearing swollen, terminal spores, "drumsticks," by growing the organism in compressed air; and still another race destitute of virulence by cultivating it at temperatures above 40°C. These are not exceptional cases, similar care being necessary in all directions if one would avoid erroneous conclusions.

Naturally, every successful experimenter will vary his culture media in all sorts of ways in order to learn as much as possible of the organism under consideration, but at the same time he will determine its behaviour on the standard media, and will keep a very careful record of all that he does. The bacteriologist should make it an invariable rule to repeat every experiment two or three times, at the very least, and generally after an interval of some months or years he should repeat all his experiments. Even then he will fall into errors enough. He certainly should proceed with as much care as the chemist, and in many directions the work passes naturally over into chemistry. If quantitative or volumetric analysis requires all sorts of precautions and excess of care to avoid errors, no less does this youngest of all the sciences.

A few words respecting the occurrence of bacteria in normal plant tissues will be in place before concluding these general remarks. It goes without saying that such minute and universally distributed bodies as bacteria are likely to be found at times almost anywhere, even in plant tissues which seem to be healthy, just as they may sometimes occur in the blood stream of healthy animals, but they are not normally present in the tissues of plants. All carefully conducted experiments have led to this conclusion. The reader who wishes fuller information may consult papers by Laurent,[14b] Buchner,[15] Lehmann,[16]

[14b] (***14***) Sur la pretendue origine bacterienne de la diastase. *Bull. de l'Acad. roy. de Belgique*, T. X., pp. 38–57.

[15] (***15***) Notiz betreffend die Frage des Vorkommens von Bacterien in normalen Pflanzengewebe. *Muench med. Wochenschrift.*, 1888, pp. 906–907.

[16] (***16***) Erklarung in Betreff der Arbeit von Herrn Dr. Hugo Bernheim, etc. *Ibid*, 1889, p. 110.

Fernbach[17] Vestea,[18] Kramer,[19] and Russell.[20] Even when purposely introduced into living tissues they refuse to grow or spread but little and finally die out,[21] unless they possess specific pathogenic power in which case the result is quite different.

The diseases which will be discussed in the following pages may be divided into four classes:

(1). Diseases of clearly established bacterial origin.

(2). Diseases which appear to be constantly associated with bacteria and which are probably due to some specific organism, but full proof of which has not been furnished.

(3). Diseases said to be more or less closely associated with the presence of bacteria and ascribed thereto, but in which little or no proof has been brought forward to establish the causal relation.

(4). Communicable diseases which have been ascribed to bacteria but associated with which no organism has been found and which are probably of non-bacterial nature.

On the whole it would perhaps be more logical to divide the following pages into four chapters in the way I have specified, but for practical reasons it has seemed better to discuss all of the diseases of a given plant in one place. I have, therefore, arranged the material by hosts, but will at the close try to summarize the whole subject in the manner above indicated.

It will certainly be some time, probably many years, before we have anything like a permanent scheme of classification for the bacteria. Our knowledge is still too incomplete. Meanwhile, we have to do the best we can with the present systems, all of

[17] (***17***) De l'absence des microbes dans les tissus vegetaux. *Annales de l' Inst. Pasteur*, 1888, pp. 567–570.

[18] (***18***) De l'absence des microbes dans les tissus. *Ibid.*, 1888, p. 670–671.

[19] (***19***) Bakteriologische Untersuchungen ueber die Nassfäule der Kartoffelknollen. *Osterreichisches landw. Centralb.* I, Heft 1, 1891.

[20] l. c.

[21] Lominsky: (***20***) On the parasitism when introduced into plants of some disease-causing microbes (Russian). Wratch., 1890. No. 6, pp. 133–135.

Russell: l. c.

Kornauth: (***21***) Ueber das Verhalten pathogener Bakterien in lebenden Pflanzengeweben. *Centrb. f. Bakt., Parasiten-Kunde, u. Infectionsk.* I Abt., Bd. XIX, No. 21, 8 Juni, 1896, pp. 801–805.

which are more or less arbitrary and unsatisfactory, and all of which are liable to be set aside at any time. I have here adopted Migula's system[22] which seems to me very convenient, and on the whole the most satisfactory of any that has yet appeared.

Before proceeding to the body of this review it only remains to say that every effort has been made to deal impartially with the material in hand, and to present the essential ideas of the writers as concisely and accurately as possible. To this end the original papers have been consulted in every instance, unless otherwise stated in the text. So much vexation over wrong references has been experienced in time past by the writer that he has himself been at special pains to give full and accurate citations. It is to be hoped, therefore, that the reader will have no difficulty in finding the original papers. An endeavor has also been made to bring the subject fully up to date but it is quite likely that some worthy papers may have been overlooked, owing to the many languages and the ever increasing number of places of publication.

THE MEANING AND STRUCTURE OF THE SO-CALLED "MUSHROOM BODIES" OF THE HEXAPOD BRAIN.

By F. C. Kenyon, Ph. D.[1]

In looking at a series of sections of the brain of a hexapod, especially of a hymenopteron, the most notable structures are two pairs, one to each side, of large cup-shaped bodies of "Punkt substanz," or, what in the light of our present knowledge of nerve structure is better denominated fibrillar substance. Each of these cups is filled to overflowing with cells having large nuclei and very little cytoplasm. From the under surface

[22] Migula: Schizophyta: (22) Schizomycetes. *Die Natuerlichen Pflanzenfamilien* (Engler u. Prantl). I Teil. 1 Abt. a, Lief. 129. 8vo. p. 44, Leipzig, 1896. This is the forerunner of a larger work soon to be published by Gustav Fischer, Jena.

[1] Clark University, Mass.

each of these cups or "Becher" there descends into the general fibrillar substance of the brain a column of fibrillar substance which unites with its fellow of the same side to send a large branch obliquely downwards to the median line of the brain and an equally large or larger branch straight forwards to the anterior cerebellar surface. (Fig. B.)

Long before our present methods of sectioning and staining had found general application in the study of animal structure, or as early as 1850, the French naturalist, Dujardin, discovered these bodies in transparent preparations *in toto* of the brains of certain Hymenoptera and Orthoptera. From their somewhat folded appearance he describes them as "lobes à convolutiones," and compared them with the convolutions of the human brain, and even thought them associated with hexapod intelligence. Fourteen years later, Leydig, using the same methods confirmed Dujardin's discovery in working with the brains of the ant, bee, and wasp, and described them as "gestielter Körper." In 1875 Rabl-Rückhard identified the bodies in *Gryllus italicus*, *Locusta viridissima*, and *Dycticus verrucornis*, and correctly described the form of the "cup" under the term "Rind Körper." The very next year ('76) Dietl's application of the section method to the subject confirmed and perfected previous descriptions, and, struck with the resemblance to mushrooms, he adopted the name of "Pilzhutförmiger Körper," a conception later used by Packard (mushroom bodies) and by Bellocici ('82) (corpo fungiformo).

As to the intellectual function of the bodies, not all of the early writers supported Dujardin's inference. They were supposed to be connected with sight; but Rabl-Rückhard showed that they are fully developed in a blind African ant (Typhlopone). Dietl was loth to acknowledge an intellectual function, even though he found the organs more highly developed in Hymenoptera than in Orthoptera. But Forel ('74) adhered to Dujardin's supposition, and showed that among Hymenoptera even of the same species the bodies are most prominent where one usually recognizes most intelligence, as in the worker bees and ants, while they are small in the females and the males. Brandt ('76) two years later in a note on the brain of Hymen-

optera makes the same observations as to the differences in the same species, while Berger ('78) considered the structures as "organs of projection of the first order."

The supposition of Dujardin obtained its best support so far as the older methods would avail in the comprehensive work of Flögel ('78) covering the whole group of hexapods. Here, one may see at a glance that the development of the structures largely coincides with the development of intelligence, as shown by the following abridgement of his table:

A. The four cups completely developed.	
1. Very highly developed,	*Vespa.*
2. Large with rim,	*Apis, Formica Pompilus, Ichneumonidæ.*
3. Without rim,	*Blatta.*
4. Very small,	*Cossus, Sphinx, Vanessa.*
B. Cups incomplete.	
5. Walls and cells so reduced as hardly to be recognized as cups,	*Tenthredo, Cynips.*
6. Reduced to two small heaps,	Many small butterflies.
7. Wall a broad plate,	*Forficula, Acridium,* etc.
8. Wall (fibrillar substance) absent.	
(*a*) Cells in 4 groups,	*Dycticus.*
(*b*) Cells in 2 groups, distinguishable by comparison with neighboring cells,	*Aeschna.*
(*c*) Not so distinguishable,	*Tabanus.*
C. Cups unrecognizable even as rudiments,	*Hemiptera.*

If such a superior neural function is indicated by the testimony and work of the earlier writers, it may well be asked whether recent neurological methods will bring out the structure of the hexapod brain as well as they have that of the other invertebrates and that of the vertebrates, and whether they will lend this view support. First, it may be noted that the physiological experiments of Binet ('94), which are those of

Faivre very much bettered, demonstrate that a hexapod may live for months without a brain, if the subæsophageal ganglion, or better, ventro-cerebron, is left intact, just as a vertebrate may live without its cerebrum. Faivre long ago showed that this ventro-cerebron is the seat of the power of co-ordination of the muscular movements of the body. Binet has shown that the brain is the seat of the power directing these movements. A debrainedhexapod will eat when food is placed beneath its palpi, but it cannot go to its food even though the latter be but a very small space removed from its course or position. Whether the insect would be able to do so if the mushroom bodies only were destroyed, and the antennal lobes, optic lobes, and the rest of the brain were left intact, is a question that yet remains to be answered. In Binet's experiments neither olfactory nor visual stimuli can be transformed into motor impulses. Were it possible for them to be so transformed, my studies to be noted in a moment cause me to think that Binet's results would be very materially altered.

Now, as to my studies. During the winter just past with no little patience I endeavored to apply the bichromate of silver method to a study of the brain and general nervous system of the common honey bee, the more detailed result of a portion of which will be published a little later. The endeavor was rewarded by a considerable degree of success, the main facts being determined, though there are many details left for future studies. Others have tried to employ the same general method, but owing to a lack of proper store of patience or to their setting about the task wrongly have failed. Among them must be counted Binet ('94), with whom, however, there seems to be a defect in the conception of both the Golgi and the Erlich methods. For he sets the former aside as inconstant, uses the latter, without, however, apparently obtaining any very good results. He complains that preparations by the Erlich method (and the Golgi method might be included) leave out many details, and never seems to think that a sufficient number of preparations will supply those details and thus allow the whole to be determined. This is the more unfortunate, since his dependence upon the old methods has led him to give detailed

importance to phenomena that are relatively unimportant, and has resulted in a somewhat misty conception of the structure of the hexapod ventral nervous system.

One of the very first things that an impregnation of bee brains with bichromate of silver enabled me to make out was the structure of the mushroom bodies with their cells. These cells stand out in sharp contrast to all other nerve cells known, though they recall to some extent the cells of Purkinje in the higher mammals. Each of the cells contained within the fibrillar cup seeds a nerve process into the later, where it breaks up into a profusely arborescent system of brahchlets, which often appear with fine, short, lateral processes, such as are characteristic of the dendrites of some mammalian nerve cells. Just before entering the fibrillar substance a fine branch is given off that travels along the inner surface of the cup along with others of the same nature, forming a small bundle to the stalk of the mushroom body, down which it continues until it reaches the origin of the anterior and the inner roots mentioned at the beginning of the paper. Here it branches, one branch continuing straight on to the end of the anterior root, while the other passes to the end of the inner root. Throughout its whole course the fiber and its two branches are very fine. Nearly the whole stalk and nearly the whole of each root is made up of these straight parallel fibers coming from the cells within the cup of the mushroom bodies. What other fibers there are enter these bodies from the side, and branch between the straight fibers very much as the dendrites of the cells of Purkinje branch among the parallel fine fibers from the cells of the granular layer in the mammalian cerebellum. These fibers are of the nature of association fibers.

From the olfactory or antennal lobe, from the optic ganglia there are tracts of fibers that finally enter the cups of the mushroom bodies as shown by Viallanes and by my studies with the Golgi method and also with a Formol-copper-hæmatoxylin method of staining. Besides these tracts the Golgi method has enabled me to make out another tract, unknown before, passing down the hinder side of the brain from the cups to the region above the œsophagus, where it bends forwards and comes in

contact with fibers from the ventral cord, which exists, although Binet was unable to discover any "growth of fibers connecting the cord with the brain."

The fibers entering the cups from the antennal lobe, the optic ganglia, and the ventral region, spread out and branch among the arborescent endings of the mushroom body cells.

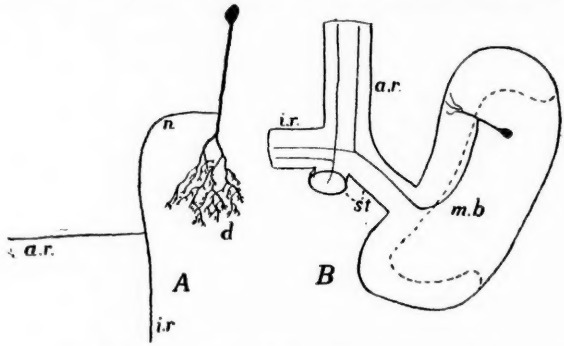

Fig. .—A. An "intellective" cell from the mushroom body. n, neurite; d. dendrite; a.r., anterior branch of the neurite; i. r., inner branch of the neurite. B. Mushroom body of right side from above. The outer one, m.b., viewed in section; the inner one is cut off, leaving the stump of the stalk st. a.r., anterior root; i.r., inner root; m.b., cup.

The fibers branching among the parallel fibers of the roots and the stalk lead off to lower parts of the brain, connecting with efferent or motor fibers, or with secondary association fibers, that in their turn make such connections. This portion of the circuit has not been perfectly made out, though there seems to be sufficient data to warrant the assumption just made.

Such fibers existing as described there is then a complete circuit for sensory stimuli from the various parts of the body to the cells of the mushroom bodies. The dendritic or arborescent branches of these cells take them up and pass them on out along the parallel fibers or neurites in the roots of the mushroom bodies as motor or other efferent impulses.

This, however, is not all. For there are numerous fibers evident in my preparations, the full courses of which I have not been thus far able to determine, but which are so

situated as to warrant the inference that they may act as association fibers between the afferent fibers from the antennæ, optic ganglia, and ventral system and the afferent fibers. There is then a possibility of a stimulus entering the brain and passing out as a motor impulse without going into the circuit of the fibers of the mushroom bodies, or, in other words, a possibility of what may be compared to reflex action in higher animals.

It appears then that the supposition of Dujardin is well supported by the finer structure of the hexapod brain. For it is evident from the details known since the publication of Flögel's paper, that the cells composing the mushroom bodies have been very highly differentiated in some of the hexapods, and this in just those forms living the most complex lives. No such bodies are to be found in the lower forms. I have never seen them, nor any indication of them, in the Thysanura, Chilopoda,[2] Scolopendrella, the Pauropoda and other Myriapoda, nor in any of the Crustacea that I have thus far examined. Without doubt an application of the Golgi or methylen blue methods would reveal elements in some these forms that might be compared with the cells of the mushroom bodies; but they would probably be found not so completely differentiated from other fibers as they are in the honey bee and other Hymenoptera. It may be mentioned that one does not recognize such cells in the cray-fish and the crab as figured by Retzius and Bethe. And it scarcely need be said that no such elements are shown in Retzius' figure of the brain of *Nereis*.

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EDITOR'S TABLE.

The Zoological Section of the American Association for the Advancement of Science at its meeting in Springfield, Mass. in August, 1895, adopted a series of resolutions which are printed in the volume of the Proceedings recently issued (p. 159) and which are here reproduced. They were adopted with but one pertinent objection from a distinguished member of the section. This objection was that the method of determining priority of publication recommended in the resolutions was applicable to questions of nomenclature only, which was regarded as an object of a value secondary to the determination of date of discovery of matters of fact. While the fixing of date of the latter was admitted to be of great importance, it was contended by the friends of the resolutions, that the manner proposed by them was applicable to all possible cases, and that in fact the resolutions prescribed the best method of determination of priority. The mode proposed was stated to be in accord with that customary among authors and publishers generally, and that special groups of authors could not in practice sustain rules different from them. The resolutions are as follows.

Whereas: The date of publication is a question of fact to be determined by examination, and not by an arbitrary ruling: and

Whereas: In the world at large the date of publication of books is the date at which they are printed; and

Whereas: The adoption of any other date of publication would have no practical effect for this reason, and for the following additional reasons; viz.:

First; the majority of publications are not distributed, but are sold;

Second; the distribution when it occurs may be rendered ineffective by accidents such as loss of mails, fires, etc.;

Third; distribution by individuals may be delayed or prevented by absence from home, sickness or death;

Fourth; distribution by governments of their publications is often delayed for routine reasons;

Fifth; the actual date of mailing will be often impossible to ascertain with precision, owing to lack of record and irregularity in the period of transmission; and

Whereas: The determination of the date of printing will generally depend on the records of the printing office and the testimony of several persons, while the time of mailing will be known generally to but one person;

Resolved: *First.*—The section of Zoology of the American Association for the Advancement of Science recommends that the date of the completion of printing of a single issue be regarded as the date of publication;

Second.—That the Section recommends that such date be printed on the last signature of all publications, whether books periodicals or "separates."

Resolved: (1) That the Section of Zoology of the A. A. As. S. is impressed with the desirability of introducing the custom of placing all publications on record at some central agency together with the date of publication. (2) That a committee be appointed to obtain the approval of these resolutions by publishing societies at home and abroad. (3) That a copy of these resolutions be transmitted to the British Assoc. Adv. Science; the Zoölogical Society of London; Australasian Assoc. Adv. Science; Association Francaise; Société Zoologique de France; Versamml. der Deutscher Naturforscher, n. Aertzte; Zoologisches Gesselschaft; and the International Congress of Zoology held at Leyden.

To act as the committee above referred to, the President of the Section appointed: S. A. Forbes, Champaign, Ill.; E. A. Birge, Madison, Wis.; W. A. Lacy, Lake Forest, Ill.; George Dimmock, Canobie Lake, N. H.

The above resolutions were adopted by very large majority vote. A proposition to regard as the date of publication, the date of receipt at the central agency of record was introduced. This was not approved, as it was evident that no private arrangement made by naturalists could supersede the customs long since current in the world of authorship.

The American Association for the Advancement of Science has a peculiar custom which it seems to us might be improved. This is the use of the term vice-president to designate the presidents or chairmen of the respective sections. This expression gives use to confusion, as these officers are not the vice-presidents of the sections, but the presidents. If the expression vice-president of section so and so is used, a president is supposed, who does not exist. To avoid conflict with the title of the president of the Association, the term chairmen might perhaps be used for the so-called vice-presidents, but actual presidents of the sections.

The decimal system of record, called the Dewey system in library catalogues, appears to the management of the Naturalist to be the best method which has yet been devised. It, therefore, follows Natural Science and La Revue Scientifique in adopting it.

RECENT LITERATURE.

The Structure of Solpugids.—That indefatigable student of the Arachnida Mr. Henry M. Bernard has presented us with a valuable account[1] of the general structure of these little known forms. And yet while we can praise the statement of facts, as a whole, we would point out that the paper contains a number of theoretical points, which have, in our estimation, no sufficient basis.

The Galeodidæ, of which over 50 species have been described, are confined to the warm portions of both hemispheres, and though abundant in certain regions, they are comparatively rare in collections; possibly from the fact that they are, by popular consent, accorded most poisonous qualities. They, alone of all the Archnida, show a distinct "head" while they also have a "thorax" divided into three segments, and these points have led many authors to look upon them as forming a transition between the Archnida and the Hexapods. They also possess stigmata in the thoracic region, a condition only paralleled in the Arachnida in certain of the mites.

In his paper Bernard takes up first the external anatomy and the interesting features here are: the interpretation of the cephalic lobes as the lateral regions of the first segment which have been changed in position with the transfer of the cheliceræ; and he further tries to find them in the cephalic lobes of embryos of other Arachnids, a view with very little in morphology to support it. The beak is interpreted as fused labium labrum, neither of these, as the name of the first might imply, being appendicular in nature. The ocular tubercle is regarded as the only remnant of the original dorsal surface of the head, the rest having been displaced by the upward and backward movement of the cephalic lobes; and, from this, the median eyes are regarded as the more primitive, the lateral as secondarily acquired. The descriptions of the limbs, as well as of the apodematous skeleton affords little to abstract, except that the author suggests that since specialized poison organs are absent the poison may come from setal-pores on the cheliceræ; and that, at any rate, the idea of their poisonous nature should not be set aside without further experiment. As little need be said of the account of the hypodermis or of the muscular systems.

The account ef the nervous system is disappointing. Although sections were cut (cf., p. 345) no use of them appears to have been made

[1] Trans. Linn. Socy. London, Zool. Vol. vi, pt. 4, 1896.

in the study of the topography of the system and we are left absolutely in the dark as to the presence of ganglia in front of those of the cheliceræ; a point of no little importance. The eyes receive hardly more satisfactory treatment, owing to the unsatisfactory condition of the specimens. No vitreous body was found in the median eyes while the retinal cells showed no rods, and no grouping of these into a rhabdem was seen. The lateral eyes vary in size, shape, and arrangement and are described in some cases as having fused on either side of the head, although no evidence is presented of such fusion. The pedipalpal organs, reversible sacs on the tips of these appendages are described in detail and are clearly sensory as are the "racquet organs" on the last pair of thoracic appendages.

The alimentary canal opens by the mouth at the end of the beak, the opening being fringed with a strainer of bristles, while the œsophagus, in front of the œsophageal collar, is modified into a "sucking stomach." The midgut is provided with gland, like diverticula and although they are grouped into those of the cephalothorax and abdomen, all clearly belong to one series, but those of the abdomen are remarkable not only from the number but from the fact that they empty into a collecting duct on either side and these ducts, in turn, empty into the intestines near the base of the abdomen. The Malpighian tubules are well developed and are described as emptying into the midgut, and Bernard accepts the views of Loman that these organs in the Arachnids cannot be homologous with the similarly named structures in the Hexapods. The heart has retained 8 pairs of ostia, while there are indications of another segmental chamber in front. From in front an aorta carries the blood forward and "appears to discharge the blood directly on to the central nervous system. There are no indications of the circumneural vessels like those of the Scorpions and of which Mr. Bernard holds, in some respects, peculiar views.

The respiratory system affords more that is interesting. The observations of previous students that there are three pairs of stigmata (and sometimes a fourth unpaired) is confirmed. Of these the first pair open behind the coxæ of the second pair of legs while the others compare with the anterior pulmonary openings of the Scorpions. Arguing from the conditions of the blood-vessels (and more from his preconceptions of the phylogeny of the respiratory organs Bernard concludes that there were originally two other tracheal openings in the thorax. There then follow some interesting but inconclusive remarks upon the primary number of stigmata in different Arachnids. While dealing with these respiratory structures the author deals with the question of

the origin of tracheæ from lung books (p. 375) and accepts the view that the former were the more primitive, the latter secondary, and reinforces it with the remark that this view "arrived at by comparative morphology, has recently been confirmed by embryology. Janorowski has discovered that the tracheal invaginations of Spiders first from branched tracheal tubes and that the lung books are a secondary specialization." And this without the slightest reference to the results of Simmons (since amply confirmed by Purcell and Brauer) which are directly the reverse. It is to be said in passing that the thoracic stigmata of the Solpugidæ, like those of the Acarina, are the greatest difficulty presented to those who believe in the Limulus-Arachnid theory, but the author dismisses the results of Wagner in this connection with the remark "that all conclusions based upon transitional phenomena of single specialized types will have ultimately to be tested by a profounder and more extended comparative study of existing forms."

The coxal glands, naturally have much attention. The external opening occurs between legs 3 and 4, the duct is long and convoluted while the gland itself is described as a great mass of tubules. These organs he is still inclined to think the derivatives of setiparous sacs, a view which "has hitherto met with no favor." Regarding the fact that they may be cœlomic in character he merely refers to Lauries observations on the scorpion and says that until this be confirmed the bulk of evidence seems to point to the coxal glands as a blind ending tube. And again (p. 381). "I freely admit that these arguments would have but little weight as against direct embryological evidence, if that evidence were really satisfactory." Certainly the results of Grobben, Kishenonyi, Lebinsky, Kingsley and especially those of Brauer are confirmative of those of Laurie, all showing the coxal glands are derived from the cœlomic wall and are the purest of mesoderm (if there be such a layer) and that their external opening is a subsequent formation. For the opposite view, held by Bernard, there seems not the slightest evidence.

After a few remarks upon the genital organs the author presents an attempt to elucidate the phylogeny of the Arachnida, and it is here that we are most at variance with him. It is impossible to go into his argument in detail. It all rests upon the attempt to derive every existing Arthropod structure from structures already present in the annelid ancestor, setiparous sacs apparently playing the most important point. These coxal glands, tracheæ, poison glands, stink glands, spinning glands cement glands, maxillary glands, salivary glands, etc., are all referred

back to the setiparous gland of the annelid; yes further, the hairy bodies of the Solpugids and Mygalidæ are direct inheritances from the annelid setæ. Scorpio is not primitive but rather a specialized form. In some of his statements of fact he also seems to be in error. Thus he says (p. 398) "What actual evidence we have as to the character of the abdominal limbs [in the primitive Arachnid] shows that they were filamentous jointed appendages like those on the cephalothorax." On the contrary in Scorpions (cf. Brauer, Patten) which, with all deference to Mr. Bernard, we continue to regard the most primitive of existing Arachnids, they appear in the embryo as flat lamellate limbs. Again (same page) he says that the sensory plates on the pectines of the scorpion are on the ventral and not on the posterior face of the limb. On the contrary they are on the posterior side as the figures of both Patten and Brauer show. But what we have most to criticise is the failure to refer to opposing views or corrections of previous statements. Thus he refers to "stigmatic scars" along the whole length of the abdomen of the Pseudoscorpions, scars which bear another interpretation. He speaks of the entostemite as ectodermal, without stating that a portion of it is mesodermal (Schimkewitsch), while we have referred to other cases above.—J. S. K.

The Bears of North America.[1]—A new classification of the bears of North America is proposed by Dr. Merriam. This classification is based on the study of more than 200 skulls, including about 35 skulls of the huge bears of the Alaska coast region. The number of full species recognized by Dr. Merriam is ten: 4 of the Black Bear group; 2 of the Grizzly group; 3 of the big brown bears of Alaska, and the Polar bear. Four of these species are new; (1) the gigantic fish-eating bear of Kadiak Island and the Alaskan Peninsula, *Ursus middendorfii* Merr.; (2) the large brown bear of Yakutat Bay and the coastal slope of the St. Elias Alps, *Ursus dallii* Merr.; (3) the large brown bear of Sitka and the neighboring islands, *Ursus sitkensis* Merr.; and (4) the Florida Black bear, *Ursus floridanus* Merr. The Sonoran Grizzly and the Norton Sound Grizzly are considered as subspecies only. The Alaskan bears fall into 2 distinct groups. (1) *U. sitkensis* and *U. dallii*, which resemble the Grizzlies in the flatness of their skulls, but are larger and differ from them in color and dentition; and (2) *U. middendorffii* which differs markedly from all other American types, and closely resembles the Great Brown Bear of Kamschatka. Merriam's synopsis is illustrated by figures of the skulls of the different species.

[1] (Proceeds. Biol. Soc., Washington, April, 1896.)

As an account of the North American bears this paper is far in advance of anything hitherto published.

The difficulty of distinguishing several species of the typical Ursi in North America has not been so much the absence of characters among themselves, as the intermediate position of the old world *Ursus arctus* with regard to them. Middendorff's studies of this species convinced him that it varied in size 33 per cent. of the largest dimensions, and in other respects, but he could not refer the varieties to more than one species. With these very elaborate studies as a basis, J. A. Allen and A. E. Brown in subsequent years could only see in the North American grizzly and black bears, geographical races. The fault then of Dr. Merriam's paper is, that he has not given any account of the relations of our bears to the intermediate series of the Old World.

Dr. Merriam is a genus fancier, and he bids fair to adopt all of the names of his illustrious predecessor Dr. J. E. Gray of the British Museum. Thus he adopts Gray's name, Thalarctos for the polar bear on characters which do not exist. He dallies with Euarctos for our black bear for equally poor reasons. We must admit, however, that Dr. Merriam does for the first time give satisfactory characters with which to distinguish this species from the *Ursus arctus*.

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General Notes.

PETROGRAPHY.[1]

The Eruptives and Tuffs of Tetschen.—Two interesting articles on the area of crystalline rocks east of Tetschen on the Elbe, have appeared simultaneously. The first, by Hibsch, is a description of the Tetschen[2] sheet of the map of the Bohemian Mittlegebirges, and the second by Graber,[3] is on the fragments and bombs occurring in the tephrite tuffs of the region.

The volcanic rocks of the district are interbedded basalts, tuffites, tuffs and tephrites, of which the fragmental rocks are in greatest abundance. Augitites also occur as sheets, and camptonites as dykes in upper Cretaceous marls. The older igneous rocks are granitites and diabases that are associated with clay slates, probably of Cambrian age. Analyses of each of these rocks are given but the rocks are not described in detail. The greater portion of the author's article deals with the volcanic rocks. The tuffs are composed of basaltic and tephritic fragments of the coarseness of sand in some cases, and in others of

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.

[2] Min. u. Petrog. Mitth., XV, 1895, p. 201.

[3] Ib., p. 291.

pieces several feet in diameter. These are cemented together by finer portions of the same substances, among which have been deposited zeolites, carbonates, opal and other secondary minerals. Some beds of this tuff are so filled with large fragments of basalt, tephrite, etc., that the rock composing it has been called the "Brocken Tuff." It is to the study of the fragments in this tuff that Graber's paper is devoted.

The basalts and tephrites constitute sheets and lava streams that are interstratified with the tuffs and sediments. Among the former rocks are noticed feldspathic, leucitic and nephelinic varieties, besides in several places magma-basalts. In addition to sheet basalts, dykes and chimneys of this rock have also been observed.

The rocks in all their forms are normal in their development. The author regards contact action around the chimneys as the safest criterion by which to distinguish these forms from denuded sheets and flows. The tephrites comprise hauyn-tephrites, in which hornblende and aegerine are present, nepheline-tephrite, including trachytic and andesitic varieties, and leucite-tephrite composed of phenocrysts of augite, plagioclase and grains of magnetite in a groundmass of these same components, and leucite, biotite and nepheline.

The augite consists of two generations of magnetite and augite in a glassy base. Its analysis gave:

SiO_2	TiO_2	P_2O_5	Al_2O_3	Fe_2O_3	FeO	CaO	MgO	K_2O	Na_2O	H_2O	Moisture	Total
43.35	1.43	1.54	11.46	11.98	2.26	7.76	11.69	.99	3.88	2.41	.59	=99.34

The feldspathic basalt and the andesitic tephrite are the only rocks that seem to have affected the sediments with which they are in contact. Quartzites are changed to aggregates of quartz grains in a glass matrix, where the action is not extremely severe, and to an aggregate of interlocking quartz grains where it has been intense. The article closes with an account of the detailed results of analysis of ten specimens of the volcanic rocks.

Graber's article is devoted principally to a description of the fragments found in the Brocken-tuff. These are all tephritic rocks, among which andesitic, leucitic and phonolitic types are recognized. The characteristics of the components of all these types are portrayed in great detail, especial care being given to the descriptions of the augite and the plagioclase. The phonolitic tephrite is characterized by the presence of nosean, which is in irregular grains. In the andesitic tephrite, which is the most basic variety, the porphyritic augite has an extinction angle $c \wedge C$ of 58°–62°, in the leucitic type its extinction is

52°–56° and in the phonolitic type, the most acid variety, it is 50°–53°. In each of the types labradorite and sometimes oligoclase phenocrysts are common, but the feldspar of the groundmass differs in character in the different types. In the andesitic type it is oligoclase, in the leucite variety andesine, and in the phonolitic type sanidine.

A Nepheline-Syenite Bowlder from Ohio.—Miss Bascom[4] has found in the drift near Columbus, Ohio, a bowlder which consists of nepheline-syenite porphyry. The rock is composed of large phenocrysts of oligoclase and smaller ones of nepheline, augite, hornblende and olivine in a groundmass composed of plagioclase and orthoclase laths, hornblende, biotite, augite and magnetite in a feldspathic matrix.

Crystalline Rocks of New Jersey.—In a report on the Archean Highlands of New Jersey, Westgate[5] states that the northern half of Jenny Jump Mt., Warren Co., consists mainly of gneisses with a small area of crystalline limestone, diorites, gneisses, etc. The gneisses are granitoid biotite-hornblende varieties, biotite-gneisses and hornblende-pyroxene gneisses. In the first named variety the prevailing feldspars are microcline and microperthite, and in the pyroxene gneisses plagioclase and orthoclase. The gneisses are cut by pegmatite dykes, amphibolites and diabases.

Associated with the white crystalline limestones are fibrolite and biotite gneisses, hornblendic gneiss, amphibolites, gabbros, norites and diorites, most of the latter of which show evidence of an eruptive origin. Another type of rock often found associated with the limestones is a quartz-pyroxene aggregate, in which the pyroxene is a green or white monoclinic augite. The limestone, the fibrolite and biotite gneisses and the quartz-pyroxene rock are thought to be metamorphosed sediments.

Simple Crystalline Rocks from India and Australia.—Judd[6] gives us an account of several simple crystalline rocks from India and Australia. One is a corundum rock composed principally of corundum grains with rutile, picotite, diaspore and fuchsite as accessory constituents. The corundum is in part pale colored and in part strongly pleochroic. The grains of the latter extinguish together producing with the former a micro-poicilitic structure. One of the specimens examined came from South Rewah and the other from the Mysore State.

[4] Journ. Geol., Vol. IV, p. 160.

[5] Ann. Report State Geol. of New Jersey for 1895. Trenton, New Jersey, 1896, p. 21–61.

[6] Mineralogical Magazine, Vol. XI, p. 56.

Associated with the corundum in the Mysore State is a fibrolite rock. A tourmaline rock from the Kolar gold field in the same State and from North Arcot and Salem in Madras, consists of twisted and bent tourmaline fibres in a matrix of smaller fibres of the same substance. In the neighborhood of Bingera, New South Wales, two rocks are found as dykes cutting serpentine. One consists almost exclusively of green garnets and the other of picotite. The former contains also gold and chrysocolla.

The Weathering of Diabase.—Mr. Merrill[7] describes the changes that have been effected in a granular diabase at Medford, Mass., during its disintegration into soil. Bulk analysis of the fresh and the weathered rock yielded the following results:

	SiO_2	Al_2O_3	Fe_2O_3	FeO	CaO	MgO	MnO	K_2O	Na_2O	P_2O_5	Ign	Total
Fresh	47.28	20.22	3.66	8.89	7.09	3.17	.77	2.16	3.94	.68	2.73=	100.59
Weathered	44.44	23.19	12.70		6.03	2.82	.52	1.75	3.93	.70	3.73=	99.81

The disintegration of the rock is accompanied by a leaching out of its most soluble constituents. Assuming that the alumnia has remained unchanged in quantity in the course of the disintegration, the percentage of each constituent lost in this process is shown to be as follows:

SiO_2	Al_2O_3	Fe_2O_3 FeO	CaO	MgO	MnO	K_2O	Na_2O	P_2O_5	Ign
18.03	.00	18.10	25.89	21.70	41.57	29.15	12.83	11.39	.00

The paper is full of valuable suggestions that cannot be even referred to in these notes.

Petrographical Notes.—Transitions from massive anorthosites into augen gneisses and into thinly foliated gneisses and transitions from olivine gabbro into hornblende schists are briefly described by Kemp[8] in a preliminary article on the dynamic metamorphism of anorthosites and related rocks in the Adirondacks.

Pirsson[9] suggests the use of the word anhedron to express the meaning usually expressed in the phrase 'hypidiomorphic form.' An anhedron is a body with the physical constitution and properties of a crystal but without the crystallographic form. The term may be conveniently applied to the crystalline grains in rock masses.

[7] Bull. Geol. Soc. Amer., Vol. 7, p. 349.
[8] Bull. Geol. Soc. Amer., Vol. 7, p. 488.
[9] Ib., Vol. 7, p. 492.

GEOLOGY AND PALEONTOLOGY.

The Limestones of the Jenny Jump Mountains, New Jersey.—Accompanying the report on the Archean Geology of New Jersey, by Mr. J. E. Wolff is a paper by Mr. L. G. Westgate on the Geology of Jenny Jump Mountain, chiefly interesting on account of the conclusions reached by the author concerning the crystalline limestones of that region.

The area under consideration embraces the northern half of Jenny Jump mountain in Warren county, New Jersey. This mountains lies along the northwestern border of the highland area, and is a sort of outlier or peninsula reaching into the later Paleozoic rocks. The main ridge of the mountain consists of gneisses; the limestone occurs at its extreme northeastern end, with outcrops along the southeast border of the mountain.

The author discusses in detail the position, lithology and relations to the crystalline limestones in other parts of New Jersey, and reviews the views of previous writers as to the age of the Sussex county limestone, which has generally been considered the type and representative of other localities. Mr. Westgate's views are given in the following summary:

"The crystalline limestones of Warren county are believed to be distinct from and older than the blue magnesian limestone of Cambrian age, which occurs along the northwestern side of the New Jersey Highlands. They are believed to be distinct, for the following reasons."

"1. They differ lithologically from the blue limestone in being thoroughly crystalline, and in containing large amounts of accessory metamorphic minerals."

"2. They are intimately associated with and apparently interbedded with the older gneisses; and gneisses occur also interbedded in the limestone."

"3. They show no intimate association in areal distribution with the blue limestone, nor any tendency to grade into it."

"4. The metamorphic changes to which the white limestones have been subjected are general in their nature, and are not due to the action of the eruptives by which they are cut; so that no sufficient agent is at hand to account for the supposed change from blue into white limestone."

"The white limestones are believed to be older than the blue Cambrian limestone, because (1) they occur in intimate association with the gneisses which are of admitted pre-Cambrian age, and because (2) they have been subjected to general metamorphic forces resulting in great changes, of which the neighboring blue limestone shows no traces."

"That the other crystalline limestones of New Jersey are of the same age as those of Warren county, has not been proved. The theory has generally been that they are. If they are, and if the position taken in the present paper is valid, then the crystalline limestones of Sussex county, and of other places in New Jersey, would also be, as they have generally been supposed to be, of pre-Cambrian or Archean age." (Ann. Rept., New Jersey State Geologist for 1895. Trenton, 1896.)

Unios from the Trias.—Four new Triassic Unios are described by Mr. C. T. Simpson. The collection of which they form a part was obtained from the Dockum beds, a formation underlying the Staked Plains of Texas. Taken as a whole, these Unios closely resemble in form, and are apparently nearly related to those of the Jurassic beds of North America, while 3 of the species bring to mind most strongly the species which now inhabit Europe and western Asia, and a small group belonging to the Mississippi area. The variety of characters displayed by these Triassic Unios go to show that the genus must have been well established at the time the Dockum beds were laid down, thus tending to overthrow Neumayer's theory that the Unionidæ were derived from the genus Trigonia, which probably does not date back to a period earlier than that of the shells under consideration. (Proceeds. U. S. Natl. Mus., Vol. XVIII, 1895.)

The Cadurcotherium.—M. Boule calls attention to the recent discovery of the lower jaw of a Cadurcotherium (Gerv.) at Barlière (Haut-Loire). The specimen denotes an animal of the size of a small rhinoceros. It was found in oligocene arkoses associated with a fine mandible of *Elotherium magnum,* and fragments of Aceratherium, and the remains of turtles. Until now Cadurcotherium has been represented by isolated teeth and fragments of mandibles. The new find is important, showing the animal to be unique among its contemporaries. It presents certain resemblances to South American types—noticably Astrapotherium of the Patagonian Eocene, but is, according to Osborn really related to the rhinocerontic genus Metamynodon.

Notes on the Fossil Mammalia of Europe, V—The Phylogeny of Anoplotherium.—The early attempts at the construction of a phylogeny of the even-toed ungulates, included the genus *Anoplo-*

therium, which was considered by Paleontologists of twenty-five years ago, as a primitive form, especially in its foot structure, *Anoplotherium* certainly possesses a number of primitive characters in its manus and pes, such as the separation of the metatarsals, and the non-fusion of the podial elements, but the inadaptive reduction of its digits, as pointed out by Kowalevsky and the peculiar position of the pollux and hallux, excludes the possibility of placing *Anoplotherium* in the direct line leading to any of the living Artiodactyla.

I propose in this short paper to attempt to prove, that *Anoplotherium* has been probably derived from *Dacrytherium*, a closely allied genus, but whose foot structure is normal and which resembles that of many of the early Eocene Artiodactyla such as *Cainotherium*. Prof Cope[1] suggested that *Cebochœrus* may have been the ancestor of *Anoplotherium*, but the structure of the skull in *Cebochœrus*, is already quite modernized, nearly as much so as in the true pigs, consequently I am inclined to think that we shall have to look for some other form as ancestral to *Anoplotherium*.

The general form of the skull in *Dacrytherium* is like that of *Anoplotherium*, however, in *Dacrytherium* there is a strongly pronounced preorbital fossa, which is absent in *Anoplotherium*. The crowns of the upper teeth in *Dacrytherium* are low and primitive in structure. They exhibit rounded external crescents, which are not at all angular. In *Anoplotherium*, especially the large species, the crowns of the superior true molars are more lengthened than in *Dacrytherium* and the external crescents are angular and broad. We see this change in many mammalian phyla from extremely low crowned molars, to those which are tending to the hypselodont condition. As regards the intermediate stage, between *Dacrytherium* and *Anoplotherium*, as to the height of the molars, this is found in the genus *Diplobune*.

The lower true molars of *Dacrytherium* exhibit two internal cones, which is the normal number in the Artiodactyla. It is interesting to record, that I have noticed in a number of young jaws of *Dacrytherium* in which the true molars were just coming through, that the antero-internal cusp, which is single in the adult, shows a slight reduplication, which is the normal condition in *Diplobune*. The division of the metaconid is carried still further in the largest species of *Anoplotherium*, although I have examined many jaws from the Phosphorites of the *Anoplotherium*, and I can confidently state, that all gradations exist between the complete isolation of the two antero-internal cusps of the typical forms of *Anoplotherium*, and the single condition of these cusps,

[1] Artiodactyla, AMERICAN NATURALIST, Dec., 1888, p. 1083.

which is found in the supposed ancestral genus, *Dacrytherium.* Accordingly I am not acquainted with any good generic character at present, which will distinguish the so-called genus *Diplobune* from *Anoplotherium*, as in many cases in jaws from the Phosphorites, it is impossible to say whether they belong to *Anoplotherium* or *Diplobune.* Dr. Henri Filhol informed me that he was of the same opinion, in regard to the validity of the genus *Diplobune.*

In Dacrytherium the hind foot has at least four well developed toes and the internal digit is not placed at an angle with the others as in *Anoplotherium.* This structure of the pes is just what one would expect to find in a genus standing in ancestral relationship to the more specialized members of the *Anoplotheriidæ.* Granting that *Dacrytherium* fulfills in most of its characters, what we require of a form, supposed to be ancestral to *Anoplotherium*, there is still the presence in *Dacrytherium* of a preorbital fossa, which is absent in the skull of *Anoplotherium*, and also another objection, is, that *Dacrytherium* has claw-like ungual phalanges, much as in *Agriochœrus.* I believe, however, the extremely compressed ungual phalanges of *Dacrytherium* is of little weight against this genus being ancestral to *Anoplotherium,* for in the latter these phalanges are rather compressed, more so than in the normal Artiodactyles, and they could be easily derived from those of *Dacrytherium.* The structure of the skull is not known in all the species of *Anoplotherium*, and one of them may have had a skull with a preorbital fossa, which is so characteristic of *Dacrytherium.*

As is well known, the original specimens of the manus and pes of *Anoplotherium commune,* which are in the Muséum d' Histoire Naturelle, Paris, show only two well developed digits as restored by Cuvier. This restoration of the feet of *Anoplotherium* is shown by Schlosser and Zittell to have been an error on the part of Cuvier, and I quite agree with these authors on this point. Prof. Zittell in his " *Traité de Palæontologie* " in speaking of the structure of the feet in *Anoplotherium* remarks "La plupart des représentation de la patte d' *Anoplotherium* faites jusqus à present omettent par erreur à la patte antérieur l' index et le rudiment de pouce, à la patte postérieur le second doigt." I have examined a fine cast of the hind foot of *Anoplotherium commune* and I find that the restoration of the internal portion as completed by Cuvier is quite erroneous. The two small bones placed by him on the tibular side of the pes do not at all fit the facets on which they are placed. The broad and obliquely placed facet on Mt. 111 in *A. commune* is for the large and wide spreading second digit, this same structure of the metatarsal occurs in *A.* (*Eurytherium*) *latipes* of the upper Eocene of Débruge.

Summing up the principal changes which have occured in the evolution of *Anoplotherium* from *Dacrytherium*, I emphasize the following: 1. Increase in height of the crowns of the upper molars, and the reduplication of the metaconid of the lower molars, this division of the metaconid is found in an incipient condition in young jaws of *Dacrytherium*. Complete separation of the metaconid into two distinct cusps only occurs in some forms of *Anoplotherium*. 2. The hind foot of *Dacrytherium* is normal in structure, and has at least four toes, this is the primitive type of pes, from which the specialized foot of *Anoplotherium* has been derived.

Note.—In my "Notes on the Fossil Mammalia of Europe," part III, AMERICAN NATURALIST, April, 1896, I find two mistakes, which should be corrected. On page 309, third and fifth lines from top, read *Adriotherium*, instead of *Adiotherium* as printed, and also page 310, eighth line from the bottom, read *Anoplotheriide*, in place of Suillines.—CHARLES EARLE.

BOTANY.[1]

De Toni's Sylloge Algarum.—Dr. De Toni[2] has recently issued the third volume of his *Sylloge Algarum*. It deals entirely with the Brown Algæ or *Phæophyceæ*—the FUCOIDEÆ as he calls them. A thousand species are described under one hundred and eighty genera, which are grouped into twenty-nine families. He divides the group into three orders, *Cyclosporinæ*, (Fucaceæ) *Tetrasporinæ* (Dictyoteæ) *Phæozoosporinæ* (Phæozoosporeæ).

Splanchnidium rugosum the interesting plant which after careful study was placed by M. O. Mitchell and F. G. Whiting[3] in the *Phæosporinæ*, is retained in the *Durvilleaceæ*, the fruit being described as a polysporous oogone. The general appearance of the plant and the structure of the conceptacles suggest a close relationship with the fucoids, but if the above investigations are to be accepted the plant

[1] Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

[2] *Sylloge Algarum Omnium Hucusque cognitarum* by J. Bapt. De Toni, Vol. III, *Fucoideæ*.

[3] On Splanchnidium rugosum Grev. the type of a new order of Algæ, Phycological Memoirs, Pt. I. I., 1892.

bears zoospores in the conceptacles and not oogones, hence it must be placed in the *Zöosporinæ.*

The treatment of the Zoosporinæ is practically that of Kjellman in Engler and Prantl's, *Pflanzenfamilien,* except that the genera *Lithoderma* and *Arthrocladia* are placed in families by themselves, instead of in the *Ralfsiaceæ* and *Desmarestiaceæ* respectively, and that De Toni has included five small, mostly, monogeneric families, the *Phæothamniaceæ, Phæocapsaceæ, Hydruraceæ, Chromonodaceæ* and *Chromophytonaceæ* not mentioned by Kjellman. In all the Zöosporinæ except the above families the zöospores as far as known are laterally biciliated and are borne in some form of zöosporangia. In these families there are no zoosporangia and in at least a part of them the zoospores are not laterally biciliated and in general their relationship seems to be with the *Chlorophyceæ.* It seems more natural to place them, as Wille has with some of them, in the *Chlorophyceæ* next to their closely related genera.

The book is well arranged; priority in class, ordinal and family. nomenclature is strictly observed. It will be indispensible to the specialist in this line and a great help to the general student.—DE ALTON SAUNDERS.

The Flora of the Black Hills of South Dakota.—In a recent number of the Contributions from the U. S. National Herbarium (Vol. III, No. 8; issued June 13, 1896), P. A. Rydberg gives the results of his explorations (in 1892) of the Black Hills of South Dakota. The report, which includes about eighty pages, includes the following, viz.: Itinerary, Geography, Geology, Altitudes, Precipitation and Temperature, Floral Districts, General Remarks, and the Catalogue of Species. The plates are a Map of the Black Hills, *Aquilegia brevistyla, Aquilegia saximontana* and *Poa pseudopratensis.* The floral districts recognized by the author are five, viz.: (1), the foothills and surrounding plains, (2), the Minnekata Plains, (3), the Harney Mountain Range, (4), the Limestone District, (5), the Northern Hills.

In summing up his discussion of the vegetation of these districts the author says, "From the foregoing can be seen what a varied flora the Black Hills have. There are found plants from the East, from the Saskatchewan region, from the prairies and table-lands west of the Missouri River, from the Rocky Mountains, and even from the region west thereof. In the foothills and the lower parts of the Hills proper the flora is essentially the same as that of the surrounding plains, with an addition of eastern plants that have ascended the streams. In the higher parts the flora is more of a Northern origin. Most of the plants

composing it are of a more or less transcontinental distribution, but often characteristic of a higher latitude. Some can be said to belong to the Rocky Mountain Region. The only trees of western origin are *Pinus ponderosa scopulorum*, and *Betula occidentalis;* the others are eastern, or transcontinental. The flora resembles, therefore, more that of the region around the Great Lakes than that of the Rockies."

It merely remains to say that the nomenclature and capitalization (*all* specific names decapitalized) of this interesting and valuable report are of the most advanced type.—CHARLES E. BESSEY.

Trelease's Hickories and Walnuts of the United States.—Dr. Trelease has rendered a good service to the botanists of the country by publishing (in the Seventh Annual Report of the Missouri Botanical Garden) the results of his studies of the Juglandaceæ of the United States, especially with reference to their winter characters. The species recognized are:

Hicoria pecan (Marshall) Britton.—Iowa to Southern Indiana, Kentucky, Louisiana and Texas, extending into Mexico.

H. myristicæformis (Michx. f.) Britton.—Arkansas to Alabama, Texas and Mexico, and in South Carolina.

H. aquatica (Michx. f.) Britton.—Virginia to Florida, around the Gulf to Texas, thence north to Arkansas and southern Illinois.

H. minima (Marshall) Britton.—Canada and Maine to Minnesota and Nebraska, south to Texas and Florida.

H. glabra (Miller) Britton.—Atlantic region from Massachusetts and Pennsylvania to Florida.—var. *odorata* (Marshall) Sargent.—Mississippi valley eastward, and from Canada to the Gulf.—var. *villosa* Sargent.—Missouri, on flinty hills.—var. *microcarpa* (Nuttall) Sargent.—Same range as var. *odorata.*

H. alba (L.) Britton.—Canada to the Great Lakes and Kansas, south to Texas and Florida.

H. mexicana (Engelm.) Britton.—Mexico, in mountains of Alvarez.

H. laciniosa (Michx.) Sargent.—New York and Pennsylvania to Iowa, Kansas and the Indian Territory.

H. ovata (Miller) Britton.—Canada to Minnesota, south to Florida, Kansas and Texas.

Juglans cinerea L.—New Brunswick to Dakota, Kansas, and the Mountains of Georgia and Alabama.

J. rupestris Engelmann.—Texas, New Mexico and Arizona, extending into Mexico.

J. californica Watson.—Coast range of southern California.

J. nigra L.—Massachusetts to Ontario and Minnesota, south to the Gulf.

The paper is accompanied by twenty five plates of trees, bark, buds, leaves and fruits.—CHARLES E. BESSEY.

Diseases of Citrous Fruits.—This recently issued bulletin (8) of the Division of Vegetable Pathology, of the U. S. Department of Agriculture, prepared by W. T. Swingle and H. J. Webber is a valuable contribution to science as well as horticulture. The diseases discussed are Blight, Die-back, Scab, Sooty-mold, Foot-rot, and Melanose. Eight good plates (three colored) accompany the paper.

Mulford's Agaves of the United States.—In the seventh volume of the annual report of the Missouri Botanical Garden, Miss A. Isabel Mulford publishes a monograph of the genus *Agave* so far as the species native to or growing spontaneously in the United States, are concerned. Sixteen species and four varieties are recognized, distributed as follows:

A. virginica L.—Maryland to Florida, Indiana, Missouri and Texas.

A. virginica var. *tigrina* Englem.—South Carolina.

A. variegata Jacobi.—Lower Rio Grande Valley, Texas.

A. maculata Regel.?—southern Texas.

A. schottii Engelm.—southern Arizona.

A. schottii var. *serrulata* n. var.—Rincon Mts., Arizona.

A. parviflora Torrey.—Mts. of Arizona.

A. lechuguilla Torrey.—west Texas and east New Mexico.

A. utahensis Engelm.—Utah, northern Arizona, southern California and Nevada.

A. deserti Engelm.—southern California.

A. applanata Lemaire.—western Texas.

A. applanata var. *parryi* (Engelm.)—southern New Mexico to central Arizona.

A. applanata var. *huachucensis* (Baker).—Huahuca Mts., Arizona.

A. shawii Engelm.—southwestern California.

A. palmeri Engelm.—southeastern Arizona and southwestern New Mexico.

A. asperrima Jacobi.—Spontaneous near San Antonio, Texas.

A. americana L.—Spontaneous in southern Texas.

A. rigida sisalana Engelm.—Naturalized in Florida.

A. decipiens Baker.—southeastern Florida.

A. sp.—Florida.

A. sp.—Texas.

It is with great pleasure that we observe the great reluctance of the author to establish new species; on the contrary she has refrained from giving names where most monographers would certainly have done so. Thus on page 96, after a description which might have been considered adequate, (at least by those who are fond of seeing their names cited in connection with specific names) the author says: "To avoid further confusion in nomenclature I refrain from giving a name to this plant until it is possible to obtain further data." We would commend this sentence to the careful consideration of a certain class of botanists who are apparently more anxious for their own "credit" than for the progress of the science.

Thirty eight plates, many of them half-tone reproductions of photographs, accompany this useful paper. If space permitted we should be glad to quote from the author's introductory discussion, which is full of interesting facts and suggestions; thus a case is cited in which the flower-stalk grew for twenty days at the average rate of two and three-fourths inches per day!—CHARLES E. BESSEY.

ZOOLOGY.

Sense of Sight in Spiders.—A detailed account of the experiments conducted by G. W. and E. G. Peckham for testing (1) the range of vision and (2) the color sense of spiders is published in a late volume of the Trans. Wisconsin Academy. The evidence offered by the authors is based upon a study of twenty species of Attidæ. This study has extended over eight successive summers, during which notes were made of many hundreds of observations. The movements and attitudes of the spiders of the group chosen are wonderfully vivid and expressive. The males, in the mating season, throw themselves into one position when they catch sight of a female, and into quite another at the appearance of another male. This power of expression through different attitudes and movements is of great assistance in determining not only its range of sight, but also its power of distinct vision.

The spiders were confined in boxes, the sides of which were marked off into inches. The bottom was of cotton cloth, the top of glass. Notes were taken of the distances at which prey was noticed, followed and captured. During their mating season the evidence was conclusive that these spiders not only see, but see clearly at considerable distance. The

following description of one of the many experiments described in the article serves to show the method of investigation:

A male of *Saitis pulex* was put into a box containing a female of the same species. "The female was standing perfectly motionless, twelve inches away, and three aud a half inches higher than the male. He perceived her at once, lifting his head with an elert and excited expression, and went bounding toward her. This he would not have done if he had not recognized her as a spider of his own species. When four and one-half inches from her he began the regular display of this species, which consists of a pecular dance. This he would not have done had he not recognized her sex."

At another time a male of *Hasarius hoyi* was dropped into a box with another male which was standing seven inches away. "He at once threw up his first legs, this being a challenge to battle. The other male responded by throwing up his first legs. The two advanced upon each other slowly, and when only two inches apart began to circle about each other, waving their legs. The same male when put into a box with a female saw her as she stood quite eleven inches away, and at once lifted his first legs, not straight up, as in the case with the other male, but obliquely, and began to move with a gliding gait from side to side, this being the characteristic display before the females in this species."

That the spiders recognize each other by sight and not by any other sense is evidently shown by the fact that they remain unconscious of each other's presence when back to back, no matter how excitable they are when they come within range of each other's vision. As a further evidence of recognition by sight a male of *Dendryphantes elegans* was removed from the box in the midst of his courtship of a female, his eyes gently blinded with paraffine, and then restored to the box. He remained entirely indifferent to the presence of the charmer that had so much excited him a few moments before.

To sum up the result of these experiments:

"The Attidæ see their prey (which consists of small insects) when it is motionless, at the distance of five inches; they see insects in motion at much greater distances; they see each other distinctly up to at least twelve inches. The observations on blinded spiders, and the numerous instances in which spiders were close together, and yet out of sight of each other, showing that they were unconscious of each other's presence, render any other explanation of their action unsatisfactory. Sight guides them, not smell."

As to a color-sense in spiders, the authors are of the opinion that their experiments, while not conclusive, yet all taken together, strongly indicate that spiders have the power of distinguishing colors. (Trans. Wisconsin Acad. Sciences, Vol. X, 1895.)

Classification and Geographical Distribution of the Naiades.—In his study of the fresh water pearly muscles, Mr. Simpson finds that the division of these mollusks into two families, Unionidæ and Mutelidæ, founded on the completeness or incompleteness of the development of the siphons, cannot stand. He accordingly diagnoses the two families on the basis of the shell characters, and finds that his distinctions fully agree with what is known of the facts of geographical distribution of the paleontology of the Naiades, and the classification of v. Ihering, based on the characters of the embryos. The Unionidæ, as defined by the author, include the genera Unio Retzius, Anodonta Lamark, Prisodon Schumacher, Tetraplodon Spix, Castalina v. Ihering, Burtonia Bourguignat, Arconaia Conrad, Cristaria Schumacher, Lepidodesma Simpson, Pseudodon Gould, Leguminaia Conrad and Solenaia Conrad. In the Mutelidæ he places the following genera:—Mutela Scopoli, Chelidonopsis Ansey, Spatha Lea, Pliodon Conrad, Brazzæa Bourguignat, Glabaris Gray, Iheringella Pilsbry, Monocondylæa d'Orbigny, Fossula Lea, Mycetopoda d'Orbigny.

The author considers the relationship between these two great groups as not a very close one. The Unionidæ are characterized by schizodont teeth and a *glochidium* embryo. The Mutelidæ have taxodont teeth, and, so far as is known, the embryo is a *lasidium.*

Mr. Simpson finds that the Naiades are capable of being grouped into assemblages of related forms which have a more or less immediate common ancestry; and on the basis of this grouping they are distributed into eight provinces, as is shown in the following table:

Palearctic, ...	Europe. Northern and Western Asia. North Pacific to the Desert. Pacific drainage of North America.
Ethiopian, ...	Africa south of the Sahara.
Oriental, ...	Asia south of the Himalayas. East Indies to the Solomon Islands.
Australian, ...	Australia. Tasmania. New Zealand.
Neotropical, ...	South America.

Central American, . .	Central America. Mexico east of the Cordillera. Cuba.
Mississippian, ...	Entire Missisippi Valley and the Gulf drainage from West Florida to the Rio Grande. Mackenzie River system. Red River of the North. Great Lakes.
Atlantic, ...	Lower St. Lawrence and rivers of eastern Canada. Atlantic drainage of the United States.

The Unios date back in America to the Trias, where they were first discoved by Prof. E. D. Cope. The relations of the existing Naiad fauna with the fossil forms is given by the author as follows:

"The post-Cretaceous Unios of the northwestern States is evidently closely related to the fauna of the Mississippi Valley, and this seems to be related to that of Tropical Africa, as well as to the tertiary forms of eastern Europe and Siberia. The Unios of Australia and South America are apparently closely related to those of the Australian region. There seems to be, too, a general relationship between the Mutelidæ of Africa and South America. These Mutelids and the Unios which bear the embryos in the inner gills have perhaps formerly occupied extensive areas in the northern hemisphere, and may have been supplanted by more modern forms." (Proceeds. U. S. Natl. Mus., Vol. XVIII, 1896.)

Arkansas Fishes.—As the result of less than three weeks' collecting in western Arkansas, eastern Indian Territory and the St. Francis River in northeastern Arkansas, Prof. Meek obtained 83 species of fishes. A new Notropis was found in the Potean River, and a new species of Fundulus is described from the St. Francis. Mollusks are abundant in old river, the old channel of the St. Francis. Six species of Unionidæ were found at a locality farther north than hitherto reported. (Bull. U. S. Fish Commission for 1895, Wash., 1896.)

Batrachia and Reptilia ot Madagascar.—The two collections of reptiles from Madagascar, now in the Natural History Museum of Paris, have been examined by M. Mocquard, who reports upon them as follows: The Grandidier collection comprises 68 species in all, Ophidians 13, Bathrachians 20, of which 3 are new species belonging to the genera Mantidactylus, Rhacophorus and Calophrynus. Lacertilians 35, including 2 new species, referred to the genera Lygodactylus

and Phyllodactylus. The Allnand and Belly collection comprise 33 Reptiles and 16 Batrachians. Among the latter are 2 new species of Mantidactylus and 1 of Stumpffia. There are but 11 Sphidia, but these include types of two new genera, Compsophis and Alluondina and a new species of Pseudoxyrhopus. The Lacertilia, 22 in number, yield 4 new species referred to the following genera: Chameleon, Brookesia, Uroplates and Paracontias. The diagnosis of the new Reptiles of this collection have been previously given in the *Comptes rendus de la Soc. Philom.* for 1894.

A comparison of these two collections, with the forms described by Prof. Boettger from Madagascar, shows that certain species considered by him as peculiar to Nossi-Bé are found distributed all through the northern part of the island. This is true not only of the Reptiles but of the Batrachians also. (Bull. Soc. Philom., Paris, 1895.)

The Molting of Birds.—In a paper published recently in the Proceeds. Phila. Acad., Mr. Witmer Stone gives a detailed account of his observations on the molting of birds, with especial reference to the plumages of the smaller land birds of eastern North America. Attention is directed to the following points: order, number and times of molt; change of color by abrasion; seasonal plumages; direct change of color in feathers. As a result of his studies Mr. Stone makes the following generalizations:

I. The annual moult at the close of the breeding season is a physiological necessity, and is common to all birds.

II. The spring molt and striking changes of plumage effected by abrasion are not physiological necessities, and their extent is dependent upon the height of development of coloration in the adult plumage, and does not necessarily have any relation to the systematic relationships of the species.

It naturally follows that closely related species may differ materially in the number and extent of their molts, and that males and females of the same species differ greatly in this respect when the nuptual plumage of the adult male is highly developed as compared with that of the female or with its own winter plumage.

III. The amount of change effected in the plumage at any particular molt varies considerably in different individuals of the same species and sex.

IV. Some species which have a well marked spring molt in their first and second years may discontinue it afterwards, when the adult plumage has once been acquired. And, on the other hand, some indi-

viduals may continue to molt in the spring, while others of the same species cease to do so.

V. The remiges are molted less frequently than any other part of the plumage. As a rule, they are only renewed at the annual molt (exception Dolichonyx).

VI. Variability in the order of molt in the remiges and presence or absence of molt in the flight feathers at the end of the first summer are generally family characters, i. e., Ceryle differs from any other species treated of in this paper in the order of molt in the primaries. All Picidæ and all Icteridæ, except Icterus (and Dolichonyx ?), molt the flight feathers with the rest of the first plumage. None of the Oscines except Icteridæ (as above), some (all ?) Hirundinidæ, Olocoris and Cardinalis molt the flight feathers at this time.

Mr. Stone's conclusions as to " color-change without moulting " are the same as those reached by Chapman, in his article on " The Changes of Plumage in the Dunlin and Sanderling," namely : that color-change without molt or abrasion is incapable of taking place from the very nature of the structure of a feather, and that all the cases so reported can be otherwise accounted for. (Proceeds. Acad. Nat. Sciences, Phila., 1896.)

The Florida Deer.—The fact that the Florida deer is but little more than half the size of the deer of northeastern United States, together with certain cranial and dental peculiarities, is sufficient, according to Mr. Outram Bangs, to give it full specific rank. He therefore describes it under the name *Cariacus osceola.* The most striking differences between the Florida animal and its northern relatives are (1) the shape and size of the nasal and maxillary bones, and (2) the very large molar and premolar teeth. (Proceeds. Biol. Soc. Washington, Vol. X, 1896.)

ENTOMOLOGY.[1]

Professor Forbes' Eighth Report.—The nineteenth report from the office of the State Entomologist of Illinois, covering the years 1893–4, has recently been issued. It is the eight report of the present incumbent, Professor S. A. Forbes, and adheres closely to the lines of thorough and accurate record, which have made its seven predecessors notable in the literature of economic entomology. The bulk

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

of the volume (189 pages) is devoted to the Chinch Bug—the arch-enemy of Illinois agriculture, a voluminous record being made of the experiments with contagious diseases carried on by the entomologist and his assistants. There is also an article on the White Ant in Illinois, and in an appendix of 65 pages Mr. W. G. Johnson, assistant entomologist, gives an excellent discussion of the Mediterranean Flour Moth.

Flies Riding on Beetle's Back.—Rev. A. E. Eaton, the well-known British entomologist, writing from Bône, Algeria, sends this interesting note to the *Entomologist's Monthly Magazine*: "Across the mouth of the Seybouse, on sandy pasture land bordering the seashore, big coprophagous beetles are common, sheltering in large holes in the soil when at rest, and running about on business. A small species of *Borborinæ* may often be seen riding on their backs, chiefly on the pronotum, and about the bases of the elytra—sometimes half a dozen females on one beetle. The beetles occasionally throw themselves on their backs to try and get rid of them by rolling; but the flies elude all their efforts to dislodge them, dodging out of harm's way into the joinings of the thorax and out again, and darting from back to breast and back again, in a way that drives the beetle nearly mad. In vain she scrapes over them with her legs; in vain does she roll over or delve down amongst the roots of the herbage; the flies are as active as monkeys, and there is no shaking them off. It is difficult to get them off into the killing bottle; nothing persuades them to fly; and they would very much rather stick to the beetle than be driven off it down into the tube."

Proteid Digesting Saliva in Insect Larvæ.—Dr. Wilibald Nagel describes[2] the method of feeding in larvæ of *Dytiscus*. In these larvæ the mouth is very much reduced in size, and the ingestion of food is performed by means of suction through the much modified mandibles, the process being facilitated by the powerful digestive action of the saliva. Under natural conditions the larvæ eat only living animals, but in captivity they will also take pieces of meat. The saliva has a marked poisonous action, killing other insects, and even tadpoles of twice the size of attacking larvæ, very rapidly. The larvæ not only suck the blood of their victims, but absorb the proteid substances. Drops of salivary juice seem to paralize the victim and to ferment the proteids. The secretion is neutral, the digestion tryptic. Similar extra-oral digestion seems to occur in larvæ of ant-lions, etc., and

[2] Biol. Centralbl., XVI, 1896, 51–57, 103–112.

spiders, and according to Krause, in Cephalopods.—*Journn. Royal Micros. Society.*

Weismann on Dimorphism in Butterflies.—For some time *The Entomologist* has been publishing a series of interesting articles by Dr. August Weismann on the Seasonal Dimorphism of Lepidoptera. The June number contains a recapitulation from which we take this extract: "Although I am far from considering the few experiments, which I could here put forward, as sufficient for reaching a decisive settlement of our opinions on seasonal dimorphism, yet I cannot forbear arranging them, provisionally at least, in reference to our general conceptions of the subject. When, in the year 1875, I first set about investigating the ways of this striking and yet so long neglected phenomenon, I assumed that it was to a certain extent obvious, that this kind of dimorphism was everywhere a direct result of the various direct influences of climate, principally of the temperature, as it effects in regular alternation the spring and the summer brood of many-brooded species. I had also well considered the other possibility, that dimorphism connected with the time of the year might also depend upon the *indirect* influence of the changing environment, *i. e.*, that it might depend upon the *adaptation* to the varying environment of the butterfly according to the time of year."

I then said: "It is not inconceivable in itself, that phenomena occur among the Lepidoptera analogous to the winter and summer clothing of Alpine and Arctic mammalia and birds, only with the difference, that the change in coloring does not arise in one and the same generation, but alternately in different ones." But, at that time the fact that the *upper side* of butterflies, which is usually not adaptive, can be very variable just in summer and spring, sometimes more so than the adaptive under side, appeared to me to contradict this adaptation of seasonal dimorphism. Yet, it was the fact, that the one or the other seasonal form could be produced artificially by the operation of a higher or lower temperature, *i. e.* the stamp of the winter form might be impressed on the summer brood, and *vice versa.* I therefore concluded that it was the measure of heat which was acting during the pupal period which directly formed the species in one way or the other; and I felt the more justified in so doing, as the climatic varieties form a parallel to the seasonal forms, and as the former must, without doubt, be referred to the direct influence of climate, especially of temperature.

Thus, for example, *Chrysophanus phlœas* is seasonably dimorphic in Sardinia and at Naples; the summer form, which develops during the

summer heat, is very dark, almost black, but the spring form corresponds with our German red-golden *phlœas*.

Although to-day I still look upon this view as correct, and a directly altering effect of temperature as proved, yet I have gradually been convinced, that this is not the sole origin of seasonally dimorphic variability, but that there is also *adaptive seasonal dimorphism*. We must, I believe, distinguish *direct* and *adaptive* seasonal dimorphism ; and, I see in this distinction an important advance, which, before all, places us in position to explain the results of the various experiments undertaken by myself and others in a much more satisfactory manner.

I have already pronounced this view in a lecture delivered at Oxford in the beginning of 1894, and I have sought to show that adaptive seasonal dimorphism, which I had previously only put forward as possible, does *actually* occur. The example there given for perfect insects was, indeed, only a hypothetical one, *viz.*, the case of *Vanessa prorsa-levana;* but for larvæ, at least, I can select an example from Edward's excellent work on the North American butterflies with tolerable certainty, *viz.*, that of *Lycœna pseudargiolus*, which will be more accurately discussed later on. I did not then know what I learnt shortly afterwards from an interesting little pamphlet of Dr. G. Brandes, that cases of seasonal dimorphism had been known for a long time among *tropical* butterflies, and that among these, at least, one of the seasonal forms depend upon the assumption of a special protective coloring. Brandes maintains, with justice, that the view hitherto widely held among us is erroneous, according to which seasonal dimorphism was not to be expected in trophical countries, since the alternation of seasons is absent there. Periods of rain and drought, at least for many tropical countries, form such an alternation very sharply. At any rate, Doherty, and, somewhat later, de Nicéville, have pointed out, for Indian butterflies, a series of seasonally dimorphic species, not merely by the observation of the alternation of the two forms in nature, but by rearing the one form from the eggs of the other ; thus among Satyridæe of the genera *Yphthima*, *Mycalesis*, and *Melanitis*, and for the species of *Junonia*, it is accepted as proved ; and in all these cases the difference between the two forms principally consists in the fact that the one form seems like a dry leaf on the under side, while the other possesses another marking, and at the same time a number of ocelli.

Without engaging in the controversy as to the biological value of these ocelli, I do not for a moment doubt but that the coloring with ocelli is also an adaptive form, possibly protective or intimidating coloring. If one of the two forms had no biological significance, it could

no longer exist; the single adaptive one would have replaced it. But it is obvious that the appearance of complicated details of marking and color, such as ocelli are, cannot be simply the direct effect of heat or cold, drought or humidity. *These influences are not the actual causes of such formations, but only the stimulus, which sets their primary constituents free, i. e.,* induces their development, as I tried to demonstrate in the lecture above noted. As the sufficient cause of the sleep of the marmots does not lie in the cold, but in the organization of the animal which is adapted to the cold, and as the cold only brings the existing predisposition to winter sleep into play, so among these butterflies with adaptive seasonal dimorphism the display of the one or the other marking is apparently connected, partially, at least, with one of the above named outward influences, although in reference to these trophical butterflies we do not yet know to which of them.

We recognize *temperature as the stimulus to development* with the cases of seasonal dimorphism of our indigenous butterflies, as in all cases of seasonal dimorphism, which have hitherto proved experimentally, it is always high and low temperature which gives the outward impulse to the appearance of the one or the other form where this impulse did not come exclusively from *within.*

There are, therefore, two different sources of the appearance of seasonal dimorphism: on the one hand, the *direct action* of alternating external influences, *viz.:* temperature, can bring about this change in the outward appearance; and on the other hand, the processes of selection. It is therefore necessary to consider these two kinds of seasonal dimorphism separately. It will certainly not always be easy to decide between them when a particular case has to be dealt with, as at present it is not always possible to say whether a coloring or marking has a definite biological value or not. Both causes also may co-operate in in one species.

Note on the Classification of Diplopoda.—The admitted impossibility of formulating a generally satisfactory definition of the term species exists partly because systematists have used it in the greatest variety of applications, and partly because natural groups are so diverse in structure and developmental history that a scheme calculated to elucidate one may increase confusion in another. It is hence desirable in proposing or making use of a classification to recognize as clearly as possible the conceptions under which the arrangement into the various categories of natural groups has been made.

The structure and distribution of the Diplopoda make it advantageous and usually easy to arrange them into species, which are groups

of very similar individuals not connected by intermediate individuals with other groups different in details of structure, form or color. An apparent and probably sufficient cause for this is the close similarity of all Diplopoda in life-histories, habits and food. All are scavengers, able to subsist upon a variety of decaying vegetable, or even animal matter, and there has been scarcely any response to calls for special adaptations to life as parasites, commensals, or under other changed conditions. The species of Diplopoda are not only extremely local in distribution, but are generally confined to almost identical habitats, removed from which they do not long survive.

Supposing the Diplopoda to be a natural group descended from a common ancestor, we are compelled to believe that such differences as appear among them are the result of accumulated variation not greatly influenced by external selective causes. Hence, existing differences indicate in general much more remote developmental divergence than in groups which have entered more thoroughly into the struggle for existence by responding to the demands of varied conditions. In this respect the Diplopoda offer a most striking contrast to the Hexapoda, and the results are in accordance; there are more millions of species of Hexapoda than there are thousands of Diplopoda.

Having accepted a criterion of species, the classification into higher groups is perhaps largely a matter of convenience; but convenience, scientific accuracy, and the recognition of affinities, alike demand constant attention to the fact, that the value of any character depends primarily upon its constancy, not upon the apparent *degree* of divergence. This is merely the reiteration of the chief axiom of systematic science, but the abundance of systems which completely ignore this fundamental idea are evidence that much reiteration is still desirable.

While in some natural groups it seems necessary to recognize subdivisions not definable by any constant character or complex of characters in the Diplopoda, we may conveniently proceed upon somewhat better ground, and require that the genera and larger divisions shall be limited by definite structural characters.

A dichotomous classification is theoretically the only exact one, for the reason that three or more natural groups could never be expected to be separated by exactly equivalent structural differences. Practically, however, a dichotomous system is inconvenient by reason of the great number of categories necessary in properly recognizing affinities. Hence, it is not a valid objection to the usual or multifid form of classification that the natural divisions arranged under the same category are not of the same rank, that is, not remote from

each other by equal structural distances. All that can be reasonably demanded of a classification is that its groups of all ranks shall be natural ones, and that the higher the groups, the more constant, and hence fundamental, shall be the characters by which they are separated. Furthermore, it must never be supposed that the variability of a character in one group need affect its importance if found to be constant in another.

As a general policy it is evidently desirable that scientific names of all grades shall mean as much as possible. The objection to the recognition of distinct and definable genera and higher groups on account of the consequent multiplicity of names is usually to be taken as an unscientific willingness to ignore structural differences and natural affinities, in the hope of escaping additional labor. In reality the difficulty of defining groups containing unrelated members, and of becoming acquainted with such through descriptions, much exceeds the temporary inconvenience resulting from change of names.

In attempting to embody in the classification of the Diplopoda a recognition of certain structural differences found to be invariable, several natural and distinct groups of families have been recognized as orders. It is here proposed to render this classification more definite and consistent by the division of two of these orders, in the belief that the resulting groups, in addition to numerous structural differences, have long been divergent in developmental history. The orders thus to be divided are the Diplocheta and the Merocheta. From the Diplocheta it is proposed to separate the true Iulidæ and their allies, under the name ZYGOCHETA, leaving under the Diplocheta Spirostreptoidea and Cambaloidea. The Zygocheta are distinct in many characters of the gnathochilarium, in the transformation of the first pair of legs of males as clasping organs, the adnate external seminal ducts, the absence of legs from the third segment, the presence of legs on the fourth segment, and the structure of the copulatory organs of both sexes. The Diplocheta have the first pair of legs nearly or quite unmodified, the external ducts distinct, the third segment with a pair of legs, and the fourth segment footless. Notwithstanding these and other important and invariable differences, it remains probable that these two orders are more related to each other than to any third group of Diplopoda.

The other case is similar; the Merocheta will, in the restricted sense, contain numerous families allied to the Polydesmidæ, with twenty closed segmental rings; the new order CŒLOCHETA will accommodate the

Lysiopetaloidea and Craspedosmatoidea,[3] and is characterized by the greater number of segments, the free pedigerous laminæ, the seven-jointed legs, the distinct mentum, and the normal presence of eyes. In the Merocheta the apertures of the external seminal ducts are small openings in the chitinous wall of the coxæ of the second legs, connecting with internal tube of nearly uniform diameter. In the Cœlocheta the coxæ contain a large cavity, while the aperture is large, the margin pilose and not chitinous.—O. F. Cook.

EMBRYOLOGY.[1]

The Tentacular Apparatus of Amphiuma.—In the Journal of Comparative Neurology, Vol. VI, March, 1896, Professor J. S. Kingsley has written an article entitled "On Three Points in the Nervous Anatomy of Amphibians" in which he has endeavored to show that the tentacular apparatus of Amphiuma, briefly described by me (Journal of Morphology, Vol. XI, No. 2), has been mistaken for a nerve and blood vessel. I consider the discovery of this degenerate organ of too much phylogenetic importance to be consigned at once to oblivion, and, therefore, offer in this article the results of a more careful study of it.

Since histological detail is important in this investigation, I state briefly the technique. The specimen, seventy-eight millimeters in length and seven millimeters in body diameter, was hardened in Kleinenberg's picro-sulphuric and, passed through the alcohol series from seventy to one hundred per cent and returned to seventy per cent, when the head was severed and placed three days in borax-carmine, then in acid alcohol twenty-four hours, after which it was imbedded in paraffine by the usual method and cut into serial sections one twenty-fifth of a millimeter in thickness.

Figure I is magnified twenty diameters. The outlines of all the features were drawn with a *Zeiss camera lucida*. Every feature appears in

[3] From the true Craspedosomatidæ there may be distinguished the Trachygonidæ, Conotylidæ, and Cleidogonidæ, in addition to the Chordeumatidæ established by C. L. Koch in 1847. The separation of other equivalent groups will probably be necessary when a fuller knowledge of European and Asiatic forms is gained.

[1] Edited by E. A. Andrews, Baltimore, Md., to whom abstracts reviews and preliminary notes may be sent.

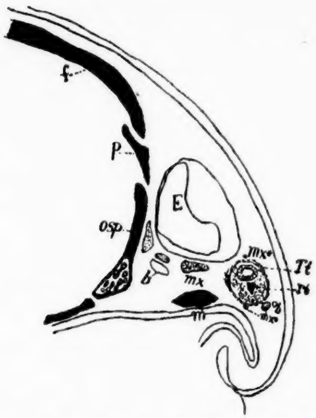

Figure I. Right-hand portion of section through head of Amphiuma 78 millimeters long, f, frontal; P, parietal; OSP, orbitosphenoid; E, eye; m, maxillary bone; mx*, branches of maxillary nerve; Tt, tentacular apparatus; rt, retractor muscle; mx, maxillary nerve.

the section just as distinctly as it is shown in the figure, b is the blood vessel and the adjacent mx* the nerve which Kingsley thought I had mistaken for the tentacular apparatus, Tt. Notice that three branches of the ramus maxillaris course along the external sheath.

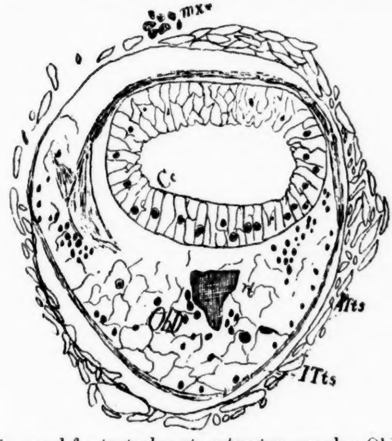

Figure II. Cc, canal for tentacle; rt, retractor muscle; ObD, orbital gland; ITts, inner sheath; ATts, outer sheath.

The histological details of the apparatus Tt. are shown in figure II as they appear viewed with a $\frac{1}{12}$ inch oil immersion lens giving about 1000 diameters. While the columnar epithelial cells lining the tentacular canal Cc are not so regular as one sees in a functional organ yet they are so well defined, especially in the lower portion that the observer cannot be misled as to their identity. The nucleus is visible in about one half the cells and the nucleolus is apparent in many cases. In the upper portion the cells have lost their nuclei and are in a degenerate condition. rt is a cross-section of a muscular element which I believe is the atrophied remains of the muscular retractor of the tentacle. In my preparation, only the bony and muscular tissues have taken on the very light shade of red which characterizes rt. Since the latter is certainly not a bone, I infer it must be a muscle, and if a muscle what other function could it have had than to retract the tentacle. This muscle is visible in ten consecutive sections while the canal Cc appears in greater or less completeness in thirteen sections. The black dots of various sizes seen irregularly distributed throughout the glandular tissue ObD may possibly be nuclei as they are stained a deep red or they may be scattered nerve fibres whose connection with the ramus maxillaris on its branches I have not been able to demonstrate because the degenerate glandular tissue was so loose as to be displaced in several sections. The irregular wavy lines, I think represent cell boundaries. These are visible with an enlargement of two hundred diameters in the lower portion but can scarcely be seen with an oil immersion immediately beneath the canal. ITts is the inner tentacle sheath composed of connective tissue fibres. It is clearly seen in eighteen consecutive sections. ATts represents the outer tentacle sheath which with a low power can be seen in twenty-five consecutive sections. Thus it is observed that this tentacular apparatus is about one millimeter long lying below and external to the eye.

The tentacular canal is complete in only four sections. Figure III represents the fourth section posterior to figure II. The columar epithelium has disappeared on the dorsal side where the inner sheath enters and on one side lies close to the wall, while on the other it mingles with a loose tissue T which may be the remains of a tentacle. This tentacle is prominent in six sections, in three of which the canal is complete so that the inner sheath does not enter it. The lumen of the canal varies but slightly in size. The musculus retractor rt dwindles as we pass anterior or posterior of the section shown in figure II. The glandular tissue decreases both anterior and posterior to the median section. The portion on the ventral side persists the longest, being present in thirteen

sections. The outer tentacle sheath retains the same circumference in about thirteen sections. As soon as the canal and glandular tissue have disappeared the circumference of the outer sheath lessens in both the six posterior sections and the six sections anterior to the thirteen

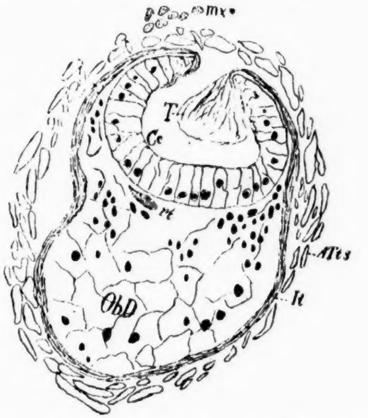

Figure III. T, tentacle; mx,* branch of ramus maxillarios; other letters same as in figure II.

median sections until it is only one fourth of the full size and the cells of the sheath become scattered, thus finally filling up the central area and creating a solid cord in the last two sections. It is worthy of notice that this tentacular apparatus was observed on the right hand side only in the specimen examined. In three other specimens of the same hatching, though they were several millimeters longer, no trace of the above described organ could be discerned. Kingsley has shown that no such organ exists in his specimens which were from the same lot as mine. An explanation of the occurrence of this organ in only one specimen may be found in the fact that it is an exceedingly transitory formation like the pronephros of the chick, which is present for only one day.

The second objection Kingsley makes to my observations, is that all the eye muscles are present in Amphiuma and the Sarasins say the retractor muscle of the tentacle is probably developed from the retractor bulbi. To this I answer that the Sarasins have not been able to demonstrate positively that the retractor muscle is developed from the retractor bulbi, and if it were true that the retractor muscle is developed from the retractor bulbi, I see no objection to the posterior part of the

retractor muscle functioning as a retractor bulbi after the anterior portion has undergone degeneration.

Kingsley further states that the described apparatus is not in the proper location to be compared to the tentacular organ of the Gymnophiona. In elucidating this point it is of service to compare figure I with figure IV taken from Die Anatomie der Gymnophionen von Wiedersheim.

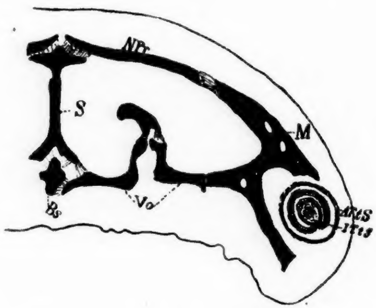

Figure IV. Cross section of *Siphonops annulatus.* NPr, naso premaxillary; Vo. vomer; M, maxillary; Atts, outer tentacle sheath; ITts, inner tentacle sheath. After Weidersheim.

It is seen that the columar-lined canal, inner tentacle sheath and outer tentacle sheath in Siphonops, have the same relation as in Amphiuma. It is further seen that the inner sheath of Siphonops is involuted ventrally to surround the tentacle while in Amphiuma a similar involution is seen on the dorsal side in Fig. III. In both genera the organ is covered merely by the skin and its subjacent tissue. The glandular tissue is not shown in Fig. IV as the section is anterior to the orbital gland. It is true the maxillary bone overhangs the apparatus in Siphonops whereas such is not the case in Amphiuma. In behalf of this contrast I quote from Cope (Bulletin of the United States National Museum, No. 34, p. 214): "There is also a very large foramen or canal passing through the o. maxillare from near its middle to the orbit, foreshadowing the canalis tentaculiferus of the cœcilia." Fig. I. is a section posterior to where the canal would enter the maxillary bone. Among the Gymnophiona there is considerable variation as to the relation of the apparatus to the maxillary bone as the following from Wiedersheim, p. 47 shows: "Sprengt man nun zum Behuf klarerer Einsicht die Deckknochen auf der betreffenden Schädelhafte volkommen ab, so wird man ein weissliches, walzenförmiges Organ gewahr, wel-

ches, wei bei Cœcilia, ganz vom Maxillarbein oder wei bei Epicrium und Siphonops an seiner äusseren circumferenz nur von der äusseren Haut bedeckt ist." Thus it is seen that the location of the organ in Amphiuma is very similar to its location in Gymnophiona.

A further corroboration of my views is noticed in the relation of the branches of the ramus maxillaris to the external sheath of the tentacle. According to Wiedersheim, in the Gymnophiona three branches of the maxillary nerve attend the tentacular apparatus in its course in the sub-orbital region. In Amphiuma I have found these three branches occupying the same relative position as is indicated by mx* in Fig. I. This striking similarity is seen at a glance by comparing fig. 54 in Wiedersheim's Anatomie der Gymnophionen with Fig. I. Before one can be convinced that the so-called tentacular apparatus in Amphiuma is really such I am aware my investigations must be verified by the discovery of this atrophied organ in other specimens. The importance of the discovery of such a feature is emphasized by Kingsley: "Were it true that Amphiuma possesses, either in the young or the adult, rudiments of a tentacular apparatus, the fact would prove of great value to those who would recognize in the Gymnophiona only degenerate Amphiumæ." Cope and the Sarasins have deduced considerable evidence favoring the close relationship of Amphiumidæ and Cœciliidæ, which fact renders it the more credible that a rudimentary tentacular apparatus has really been found in Amphiuma.—ALVIN DAVISON, PH. D.

PSYCHOLOGY.

Synæsthesia and Synopsia.—Until quite recently synæsthesia was regarded by psychologists generally as a purely artificial and fanciful association, or at best as a sign of degeneracy; it has lately received considerable attention, however, and the weight of evidence goes to show that it is both natural and normal—it may even be said, a phenomenon of common occurrence.

In an exhaustive monograph on the subject, published in 1893,[1] Prof. Flournoy of Geneva for the first time introduced a terminology which aimed to distinguish scientifically between the different forms of synæsthesia. The most important phase is the association of visual images, or *synopsia.* Attention was first called to this by Fechner, in 1876.

[1] Les phénomènes de la synopsie (audition colorée); by Th. Flournoy; Paris, 1893; pp. 259.

Flournoy distinguishes here between photisms, diagrams and personification. The first of these is the *audition colorée* of earlier writers; it consists in the natural association of a color with each particular sound, so that a spoken word appears to the hearer to be tinged with one or more hues, corresponding to its constituent vowel sounds. A diagram is a visual scheme in which some natural series of ideas (such as the months, days of the week, numbers, etc), is arranged. When a member of the series is recalled, the appropriate part of the diagram is visualized. Personification is simply the attributing of some personal characteristic, such as sex, to a number, etc.; or the association with it of a feeling of like or dislike. Flournoy reports some 350 persons as possessing synopsia in one or other of its forms, out of 2600 to whom questions were addressed, (13 per cent.); but as a large portion of his question-sheets were never returned, the real percentage may be regarded as somewhat greater.

In a recent paper,[2] Miss Calkins gives the results of a personal canvass of Wellesley students in 1893 and 1894. For the former year the affirmative answers numbered 33 per cent., for the latter 60 per cent. It may be doubted whether all the latter are true cases of synopsia. Yet when due allowance is made for possible temporary associations, it must still be admitted that synopsia is by no means a rare phenomenon.

Richard Hennig[3] gives an interesting study of the diagram-forms occurring in himself and his immediate family. He is able in a number of cases to trace their origin to certain associations of early childhood, and favors the 'natural,' or experiential view of the origin of all such schemes. He strongly opposes the notion of inherited forms or photisms: Only two pair in the list given by Galton, he thinks, show any real resemblance, and these may well be accounted for by similarity of early environment. "Only the tendency to synopsia can be inherited; but here the influence of heredity is unmistakable and undoubted." The writer points out a similarity between the number-form of himself and one brother brought up under the same surroundings, while in the case of another brother, whose early life was spent in another environment, the diagram was radically different. Herr Hennig urges the usefulness of the number-form as a mnemonic aid, and cites the case of a friend, who easily memorized dates by association with the appropriate places in his number-diagram.

J. Philippe has lately investigated the synopsia of blind persons, and finds a remarkable number of cases among them, though none occurred among those who were blind from birth.

[2] Synæsthesia, by Mary W. Calkins; Amer. Journalof Psychology, VII, 90–107.
[3] Ztschr. f. Psychol., X, 183–222.

With the reduction of synæsthesia to a scientific basis, which Flournoy has brought about, and the demonstration of its wide-spread occurrence, comes the demand for a more thorough examination of its bearing upon other departments of psychology. The physiological interpretation of synopsia is still unsettled, and is commended to physiological psychologists as a fruitful theme for investigation.—H. C. Warren.

ANTHROPOLOGY.[1]

Exploration by the University of Pennsylvania in West Florida.—Little more than a year ago my friend Lieutenant Colonel C. D. Durnford formerly of the English Army, returning northward from a journey in the West Indies and Florida brought with him the specimens of aboriginal rope and netting found in a mud bed near Marco, Florida described by him in the American Naturalist for November, 1895.

That he realized the importance of the digging done in the mud in April, 1895 by himself and Mr. Charles Wilkins of Rochester, New York, was shown by the fact that on reaching Philadelphia he made the effort at once to present the details of the discovery to archæologists. As an original observer, a gatherer of inspiration from nature, coming generously to present us with unprecedented specimens and archæological data of much value, discribing to myself and others the details of the discovery and stating his belief that the lagoon fringing islands near Marco were net-worked with artificial canals, and would disclose other and similar relic preserving mud deposits, to him belongs the honor of opening a new door for archæology in the southeast.

The prompt recognition of the originality and value of this intelligence by Dr. William Pepper and his energetic action in cooperation with Mr. Stewart Culin, Director of the Department of Archæology have resulted in the recent expedition of the University of Pennsylvania sent by Dr. Pepper to Florida in the late months, under the direction of Mr. Frank Hamilton Cushing, whose fortunate presence in Philadelphia at the time of Colonel Durnford's visit ended in his employment by Dr. Pepper as Conductor of the Exploration. This led to the association of the Bureau of Ethnology of Washington of which

[1] This department is edited by H. C. Mercer, University of Pennsylvania.

Mr. Cushing is a member, with the work whose results have delighted the friends of the University.

Summarized by Mr. Cushing in two newspapers (Philadelphia Times and New York Journal Sunday, June 21st, 1896) these results are represented by the array of specimens now in the Pepper Labora tory at Philadelphia. They witness the good fortune of Dr. Pepper and the University and the successful excavation of Mr. Cushing. The muck-filled artificial shell basin at or near where Coloned Durnford had worked, dammed, baled and cleaned out, and a large mound excavated 200 miles to the northward procured a superabundance of beautiful and unique remains.

The work shows that a storehouse of aboriginal manufactures escaping the notice of a good deal of reconnaissance, had lain unobserved within easy reach of scientific institutions in the east, testifying further to the fact that mud or permanent damp has here done for the Archæologist what permanent dryness has done at the Cliff Dwellings of Arizona and in Egypt. As at the Swiss Lake Dwellings here again, a whole category of remains that have perished elsewhere in the eastern United States have survived hermetically sealed in the ooze.

A few of the salient features of the collection concern:

(1) Facts relating to burial; crania from the mound and muck with funeral paraphrenalia.

(2) The relation of pottery, found in great abundance, to burial, and the allegoric and religious significance of fictile designs.

(3) The use of totemic ornaments, of masks representing the human face in ceremonials, and the allegorical significance of carvings representing the heads of animals, and paintings on wood.

(4) The economic facts of daily life illustrated by means of well pre-preserved utensils and vessels of wood and by the haftings of wood and shell implements.

(6) Interesting data refeiring to the arrangement of canals, shell walls, basins, the height of shell mounds and what appear to be vestiges of pile-built houses sunken in mud and sufficiently indicated for study.

It will not be easy for the archæologist suddenly confronted by this display of aboriginal handiwork outshining the long toiled for gatherings of other searchers in the East, to hold fast to the caution that the occasion demands, to realize how much and how little such preservation of perishable remains signifies in a given case, to remember in the inferred estimate of cultural status that multitudes of similar objects, betokening the life history of other tribes in the eastern United States have perished, in short to weigh considerations that must temper the use of

colored words signifying degree of ethnic importance, advanced methods of construction, superiority in the arts, and kinship to other peoples.

Meanwhile the excavation and production of the strange carvings in wood, the human masks, the unique paintings, the hafts of wood and tools of shell, the relics of rope and fabric, remain in evidence to speak in manifold praise of the enthusiastic searcher who while telling his glowing story has shown that he has known where to dig and dug with effect.

Symbols inscribed upon the drawings of birds, totemic buttons arrangements for burial with reference to the "four quarters of the world," the paraphrenalia of priests buried together in the mud here seek explanation at the hands of an interpreter, whose experience should have qualified him for the task. Luckily the elucidation of the allegorical meaning of the serpent and the raccoon, the gopher and the bat, the badger and the cormorant, tokens of gods of the dead and the living of the morn and the dusk, has fallen to the lot of one whose knowledge of the mystic inner life of the Indian, gathered upon a painful path of Zuni initiation might best recognize in the manifold characters of these remains a symbolism hidden to other eyes.—HENRY C. MERCER.

SCIENTIFIC NEWS.

The International Geological Congress will hold its seventh session, in 1897, at St. Petersburg, Russia. The presidential chair on that occasion will be occupied by M. A. Karpinsky. A number of interesting excursions have been planned to take place both before and after the meeting. It is proposed to visit Finland and the Ural country, to examine the basins of the Don, the Volga and the Dneiper. While the grand tour at the close of the Session covers the ground from St. Petersburg to the Caucasus, giving opportunity for special examinatlon of many interesting localities.

The circular of announcement gives the following information in the closing paragraph.

"The Committee on Organization takes pleasure in making known to you that His Majesty the Emperor, upon the report of his Excellency the Minister of Ways of Communication (Transportation) has deigned to grant to all the geologists (who give notice in time of their

intention to take part in the work of the Congress) tickets allowing them free first class transportation on all Russian railways before and after the Meeting of the Congress, including the excursions."

Lord Lilford, the President of the British Ornithologists' Union, died June 17, 1896. At the time of his death he was engaged in a work on the Birds of the British Islands which was nearly completed. He was a contributor to Ibis, The Zoologist and the Proceedings of the London Zoological Society. His interest in natural history led to his keeping an extensive collection of living animals at his country seat in Northhamptonshire.

Mr. T. D. A. Cockerell, Las Cruces, New Mexico, will be glad to furnish information concerning the biological station he proposes to establish in New Mexico. If a sufficient number of students are enrolled, a beginning will be made this summer. For the study of nsect life New Mexico presents an unusual combination of advantages.

The prizes awarded by the London Geological Society have been distributed as follows: The Wallaston Medal to Dr. Edward Suess, Ph. D. Prof. of Geology in the University of Vienna; Wollaston Donation Fund to Alfred Harker, M. A. of the Geological Survey of Scotland; The Murchison Medal to T. Mellard Reade, Esq.; Murchison Geological Fund to Philip Lake, Esq.; The Lyell Medal to Arthur Smith Woodward, Esq.; Lyell Geological Fund to Dr. Wm. Fraser Hume, Demonstrator of Geology in the Royal College of Science and Charles W. Andrews, Esq.; The Barlow-Jameson Fund to Joseph Wright, Esq. and Mr. John Storrie of Cardiff. (Quart. Journ. Geol. Soc. London, 1896.

Messrs. Hatcher and Peterson have gone to Patagonia to collect fossil vertebrata in the Cenozoic beds of Patagonia for Princeton University.

Macmillann & Co. have made arrangements for the issue in New York and London of a "Dictionary of Philosophy and Psychology" under the editorial supervision of Professor Baldwin of Princeton University.

The following assignments of topics with the names of the authorities who will contribute original matter may be already announced:

General Philosophy and Metaphysics.—Prof. Andrew Seth, Edinburgh University; Prof. John Dewey, Chicago University. *History of Philosophy.*—Prof. Josiah Royce, Harvard University. *Logic.*—Prof. R. Adamson, Glasgow University. *Ethics.*—Prof. W. R. Sorley, Aber-

deen University. *Psychology.*—Prof. J. Mck. Cattell, Columbia University; G. F. Stout, W. E. Johnson, Cambridge University; Prof. E. B. Titchener, Cornell University; The Editor, Princeton University *Mental Pathology and Anthropology.*—Prof. Joseph Jastrow, Wisconsin University. *Biology.*—Prof. Lloyd Morgan, University College, Bristol. *Bibliography.*—Dr. Benjamin Rann, Harvard University.

With the publication of No. II, Vol. II, of its bulletins, the Chicago Academy of Sciences enters upon a new era of activity. Its publications will be issued at regular intervals. The Academy property is now housed in a fire proof building of the best architectural construction, and no further fears of fire are entertained.

Dr. Joseph F. James begs to inform his friends and correspondents that he has removed from Washington, D. C., and that after May 10, 1896, his address will be Hingham, Mass.

I desire to secure good sets, cleaned or uncleaned, numbering fifteen or more specimens each, of your local representatives of *Campeloma* (*Melantho* of Authors), *Lioplax* aud *Vivipara.* Where extra large sets can be sent they will be of especial value since the present object is monographic. Exchanges are offered in southern *Unionidæ* and *Strepomatidæ.* The rarer forms of the last named groups are also desired.

Cincinnati, Very respectfully

1815 Fairfax Ave. R. Ellsworth Call.